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ABSTRACT

This monograph contains papers and workshop summaries from a two-day symposium for ethnographers and drug abuse policymakers. An introductory paper outlines the background and purpose of the symposium, followed by papers that provide a definition of ethnography, present a history of illicit drug ethnographies, and examine state problems and the need for research-based planning in the drug field. Other papers explore issues and problems in using ethnography in single state agencies. Research in minority communities is described, along with the use of an ethnography field station and the methodology and findings of a four-city study of PCP users. The final paper discusses ethnographic research and public policy development. The workshop summaries provide an overview of the results of the symposium. (NRB)

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ETHNOGRAPHY: A Research Tool for Policymakers in the Drug & Alcohol Fields

Symposium Papers

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This monograph is based upon papers presented at a symposium which took place on April 18-19, 1979, at the Conrad Hilton Hotel, Chicago, Illinois. The symposium was conducted and reported by Birch & Davis Associates, Inc., Silver Spring, Maryland, under NIDA contract No. 271-78-4609.

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Preface

Papers and workshop summaries in this volume are the immediate, tangible result of a 2-day symposium held in Chicago in April 1979. Approximatelv 30 ethnographers and an equal number of drug abuse policymakers from Single State Agencies attended the symposium. Officials from the National Institute on Drug Abuse (NIDA), local government representatives, and representatives of the National Association of State Alcohol and Drug Abuse Directors (NASADAD) also were in attendance.

The first day of the symposium was devoted to presentations of the papers contained herein, though some of them were presented in abbreviated form at the symposium. The second day involved workshop discussions and a summary session of those workshops. The workshop summaries, as well as the compilation of major points at the end of the volume, provide an overview of the results of the symposium.

Since 1975, NIDA has been systematically transferring the responsibility for managing the federally supported treatment network to the States. Although this action provided the State drug agencies with more authority in determining how the Federal funds are to be converted into service delivery at the local level, it also brought new tasks and responsibilities.

For example, State agencies must be in a position to determine how the funds and available resources can be best used. In doing so, these agencies also are responsible to the diverse community treatment needs. In addition, State agencies are constantly called on to produce information about particular drug problems and issues.

Drugs of abuse include opiates, synthetic narcotics, barbiturates, tranquilizers, cocaine, amphetamines, antidepressants, hallucinogens, inhalants, various over-the-counter prescriptions, and new synthetic substances such as phencyclidine (PCP). The patterns of drug use and abuse have been constantly changing as more chemical substances have become available and drug use has spread to new and younger populations. As attitudes about drug use continue to change, it is increasingly difficult to formulate appropriate treatment and prevention strategies.

To understand and at the same time be responsive to the new drug patterns and trends in their localities, State authorities

must develop new study methods and strategies. The large-scale data systems that have been established at the national level provide information on national trends but are of little use in assessing drug use at the local level. Surveys, which rely on self-report, are subject to serious limitations because people are reluctant to report illegal activities. State officials find that current information-gathering methods are simply not adequate to meet the ongoing and increasing need for information.

Ethnography, a study methodology that uses participant-observation techniques, has received considerable attention in the field as a result of recent contributions in the study of drug use. Although it is viewed as a potential resource by State authorities, there is still little known about this methodology--how it can be used by State agencies, the specific role ethnographers might play, the obstacles and risks involved, and the kind of investment required. To date, only a few States have had any experience in using ethnography.

This work symposium represents an initial effort to answer these and other questions. It brought ethnographers and policymakers together to--

- Help policymakers understand ethnography, how it is conducted, its limitations, and its strengths;
- Identify the role ethnographers might play in producing information needed by the States;
- Help ethnographers understand the State drug authorities, their peculiarities, needs, and resources; and
- Identify and perhaps anticipate some of the issues that may arise in a working relationship between ethnographers and policymakers.

The papers presented in this volume provide both important generalizations about the findings and uses of ethnography as well as much illustrative detail, which is a hallmark of skillful ethnographic studies. Though reading proceedings is not a substitute for attending the symposium, these proceedings do much to inform those who were not able to attend. To a great extent, the excitement and enthusiasm of the symposium participants is reflected in both the formal papers and in the workshop summaries.

As is usually the case in such gatherings, this symposium raised many questions which were not answered. But many answers, however tentative, were provided. The discussions, which took place between ethnographers and planners and policymakers helped members of each group to understand how the work of the other is done.

Both groups agreed that drug abuse planning and policymaking could be improved through the use of the kind of information ethnography can provide. Such information can make it possible

for the policies designed and implemented by the States to come closer to meeting the needs of the drug-abusing population.

Ethnographic studies are frequently quicker than surveys or other types of data gathering operations, especially if ethnographers are already working in an area. In addition, the ethnographer whose sole business is to "work the streets" for information can provide information that is less biased (or at least has different biases) than similar street information provided by program operators who also are close to what is happening in the streets. Ethnographers, who are by definition trained observers, recorders, analysts, and reporters of a volume of information which appears to be anecdotal, can also bring greater understanding to the dry numbers of surveys and secondary statistical sources. Thus ethnographic studies can not only increase the speed with which information is provided, but can also do much to increase the accuracy of the information.

Ethnographers have served as translators so that drug abuse policy can be changed, as the formal papers presented in this volume so amply demonstrate. Unfortunately, the changes in policy often come slowly, and the rapidly changing world of drug abusers, on which ethnographers report, too often is not the same world which policymakers invoke when making decisions and designing programs.

Just as ethnographers can assist drug abuse planners and policymakers, the planners and policymakers can assist ethnographers. The most important form of assistance that planners and policymakers can provide to ethnographers is the resources to carry on fieldwork. Such fieldwork must both be of use to the planners and policymakers while at the same time furthering the questions which ethnographers have, both methodological ones and those concerning behaviors of various groups. There are many questions which must be answered, both ethical and practical, in providing these resources. Those questions are raised in the formal papers in this volume, as well as in the discussion summaries that follow.

In addition to providing resources for ethnographic studies, planners and policymakers can also assist ethnographers in a more subtle way--one which was clearly demonstrated through the discussions at the symposium. If policy decisions and plans for implementing those decisions are made based on the world as described in an ethnographic study, clearly that study in particular, and ethnography in general is given legitimacy beyond the field of ethnography. Appropriate use of ethnographic studies can and will provide proper recognition for the importance of ethnography to groups much larger than those that are interested in ethnography for its own sake.

The symposium produced the positive exchange between the policymakers and the ethnographers reflected in the papers and the workshop summaries. The ethnographers view the State agencies as providing unlimited laboratories in which to practice

their craft. The State representatives have a pressing need for the kind of information that ethnographic studies can supply. There are many barriers to be overcome; many problems and limitations have not been adequately addressed. Yet, this symposium initiated what may be a lasting relationship between policy-makers in the drug field in need of information and those who seem to be in a position to supply some of this information.

Carl Akins, Ph.D.

WORKSHOP FOR ETHNOGRAPHERS AND SINGLE STATE AGENCY POLICYMAKERS AND PLANNERS

Chicago, April 18-19, 1979

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Introduction

Background and Purpose of the Ethnographers' Policymakers' Symposium

Harvey W. Feldman, Ph.D.

Studs Terkel, probably the best interviewer in the United States, enjoyed the asking of what he and Jacob Bronowski, the physicist, called "the impertinent question" (Terkel 1973). I would suggest that in the next 2 days, a number of us may ask many questions that are both pertinent and impertinent; and the one that is probably uppermost in our minds now is: What is the purpose of this symposium?

It is my task to explain briefly why more than 20 representatives of Single State Agencies and 25 ethnographers have been brought together. To my knowledge this is the first time that ethnographers and policymakers in the drug and alcohol fields have met to explore how ethnography might be used by State agencies as a planning tool, a means of better social planning, and a method of developing public policy in the drug and alcohol field. And, if I may be permitted to speak on behalf of the ethnographers, we welcome the occasion.

Ethnographers have been studying deviant groups since before the turn of the century and have, in recent years, developed a specialty in the study of drug users. As Dan Waldorf will describe shortly, there has developed a scattered but impressive body of literature in which researchers have gone into the streets and neighborhoods of drug users to study both them and their way of life up close.

In the past, ethnographers have had to work in their respective isolation--that is, they go about their research business in the helter-skelter manner that funding mechanisms permit. Most of us are familiar enough with this pattern to know that it requires riding the crests and troughs of foundation interests and changing government priorities. For the most part, ethnographers in drug research, like so many academics before them, have pursued their own theoretical interests--but with a difference. By spending so

much time in the company of drug users in their natural environments, ethnographers have acquired a heightened awareness of users' everyday problems, and cannot ignore them, no matter how esoteric the theoretical interests may be. Unless ethnographers resort to studying occupants of ivory towers, they are forced by their tasks to live in what most of us in academia call "the real world," which means ethnographers study drug-using groups in action, not merely the collection of clients or patients in formal institutions. For that reason, some of their observations have run counter to the shibboleths of the medical priesthood that dominate the drug field. Remember that we had been told for years that heroin addicts were fringe group members--isolated, shy, unable to form relationships--people who took drugs to retreat from life's problems into a warm, hazy comfort. Our theoretical understanding of addicts was altered forever when the action-packed life on the streets was described to us by ethnographers like Alan Sutter in "The World of the Righteous Dope Fiend" (Sutter 1966) and Ed Preble and John Casey in their article, "Taking Care of Business," which is probably the most anthologized and referenced article in the drug literature (Preble and Casey 1969).

Our purpose, today, entails a two-part goal: (1) to discuss ways that ethnographic research can be used by State and local planning agencies in the drug and alcohol fields; and (2) to explore the feasibility of establishing several ethnographic field stations as a beginning step to constructing a larger research network that can make realistic contributions to intervention, education, and prevention plans.

There are many reasons why ethnographers would be eager to participate in reaching these goals, and I would like to suggest only two that I think are important.

First, ethnographic field stations would provide a structure in which ethnographers could become more aware of each other's work and be in communication while their research is in progress. For those people not fully acquainted with ethnography, it should be noted that almost all ethnographers work alone. An ethnographer usually selects one community or one population group, then begins the process of meeting respondents in their own territory. Although there are many compelling needs during that process, one of the most important is simply someone to talk to about the problems encountered. This is particularly true for the young ethnographer, but it also applies to even the most experienced. To have someone available to talk things over with--especially someone who might be studying the same or a similar population--is like an unexpected gift of water to the man who has walked too long and too far in the desert. With ethnographic field stations, there would be--by definition--a pool of researchers in different parts of the country waiting and available to trade ideas, war stories, or to offer methodical suggestions.

Second, almost without exception, there exists in ethnographic research what could be called "the starting gate syndrome."

Unlike the quantitative researcher, the ethnographer depends on sustained relationships over a period of time to develop the trust and confidence of respondents that will insure the collection of reliable and truthful data. Of course, these relationships take time to develop. When they are developed successfully, they provide the entree to observations and discussions that truly illuminate the activities under study. Because gaining entree to groups whose members often distrust or dislike outsiders is so difficult and time consuming, it tends to be one of the ethnographer's most satisfying accomplishments.

By the time the research has been completed (usually a period of 2 to 4 years) the ethnographer has done more than develop a number of isolated friendships--she or he has become part of a whole network of relationships and has become a link, as it were, in the grapevine of local information. If she or he has been successful, access to this network is not restricted to the study population alone but ranges into literally hundreds of potential populations. This develops simply because the trust the ethnographer has cultivated with one group has a kind of contagion. By the end of 2 years, the ethnographer may well have access to literally dozens of local scenes whose members have come to know his scientific interests and trust his ethical protocol. At this point the ethnographer sits on a gold mine of data.

But, unless the ethnographer can plan far enough ahead and find the appropriate funding sources, those new studies tend to be simply the pipedreams an ethnographer's future is so frequently made of. Because most government funding sources are apparently loathe to fund any individual researcher for consecutive studies, the ethnographer who has enjoyed one successful project must relinquish those hard-won relationships and allow them to wither. With ethnographic field stations established on a permanent or semipermanent basis, that wasteful condition can be altered. It strikes me that where ethnographers have been able to maintain continuity of research in the same geographic area--such as Alan Sutter in the California Bay Area, Ed Preble in New York, Jennifer James in Seattle, Patricia Cleckner in Miami, Jim Walters in Philadelphia and Wayne Wiebel in Chicago--the quality of observation and the level of analysis have been consistently high. In reading their published material, it seems apparent that they have developed an intimate knowledge of their study groups and possess an insider's knowledge of those groups' attachments both to each other and to their surrounding world. In other words, they know their cities. In their own ways, they have already established individual ethnographic footholds, and we will look to them for greater amplification of their experiences as our symposium progresses.

These reasons for establishing field stations are from the perspective, and probably for the benefit, of ethnographers. Why would responsible bureaucrats and State planners need or want field stations of the sort we shall soon be discussing? Although I cannot claim any expertise as either bureaucrat or planner, I would like to list some unmet needs that have not, I believe, been

given their proper importance. Then I would like to suggest that ethnographic field stations would help provide either the information or the mechanism by which these needs can be met.

First, there is a need for an ongoing, stable method for identifying the introduction and use patterns of new substances as they become popular. There may be any number of opinions on what has caused the high degree of drug switching that is the present pattern of use among young people, but one phenomenon seems clear: New drugs--such as PCP--continue to hit the youth market and spread through it, apparently at approximately the same time in different parts of the country. The pattern of a series of "pop drugs" has been predicted by Laurence Carroll, Director of the Division of Resource Development at NIDA. These "pop drugs," as Dr. Carroll observed, will probably be synthetic substances made in laboratories; they will be easily synthesized from readily available supplies, requiring little initial expenses, no specialized equipment, and minimal knowledge of chemistry and laboratory techniques (Carroll 1978).

If the outlook Dr. Carroll has suggested seems designed to complicate the lives of Single State Agency directors and their State planners, Alexander Shulgin, a California pharmacologist, sees the future as being far more complex. In an article on PCP he prepared for a special edition of Clinical Toxicology, Dr. Shulgin predicts a new heroin problem:

> The largest area from which future drugs related to heroin might emerge is that which is completely independent of any opium sources . . . [a]any completely synthetic substitutes for morphine have been prepared and evaluated, and all of them are potentially available from domestic origins.

After discussing in some detail the chemical structure and potencies of these various drugs, he continues:

> If these potencies should extrapolate to humans (for which there is much precedence) and if the euphorigenic properties continue to accompany the analgesic properties..., then submilligram amounts should serve as satisfactory heroin substitutes. It must be remembered that this family, and all following materials discussed in the framework of heroin substitutes, are chemicals that are readily synthesizable from commercially available, and uncontrolled, starting materials... Many substitutes have been explored...and virtually all are equal to, or exceed, morphine in effectiveness. The potential for illicit mischief here is enormous.
>
> (Shulgin 1975)

And, if the outlook for heroin substitutes is not staggering enough, he wrote that ". . . there are just short of 4,000 chemicals already synthetically described and clinically documented in the scientific literature which might well serve as future drugs of

abuse." And these are only for heroin substitutes. If what Shulgin has suggested already does not churn up your anxieties, his article goes on to suggest a similar multiplication for stimulants, hallucinogens, and over-the-counter drugs.

With this possibility in our future, the drug game has the potential of becoming far more intricate than a three-dimensional game of chess. If we are to avoid the same embarrassing position we found ourselves in with PCP--in which the drug was in common use among adolescents for approximately 3 to 5 years without the drug treatment or drug research community being aware of it--then we shall need some mechanism to alert us to the introduction of new drugs into the youth culture. What we shall need is a kind of antenna that stretches into the streets and hang-outs of young people so that we can collect accurate information on how those drugs are being used, what adverse reactions they cause, and all the other pieces of information that are part and parcel of planning treatment, prevention, or education efforts that are somehow connected to the realities of users themselves. It strikes me that, if we had identified the negative aspects of PCP as they were seen on the street early enough, our preventive methods might have emphasized that "burning out"--the development of a spacey, hesitant, foggy, forgetful condition--was the feature of PCP that users found distressing and the violence that many experts associated with its use was not a key issue for PCP users on the streets. In fact, in our own ethnographic study, violence was an infrequent occurrence. Using violence as the touchstone for a preventive approach simply had no power of dissuasion among the street populations who used PCP. Each time those of us in policymaking positions employ not simply fear tactics, but fear tactics that have a frail factual base, we lose credibility among the audience whose behavior we are trying to influence. The ethnographic method has, we believe, the potential to help policymakers avoid such gross errors. By having regional ethnographic field stations, we shall be able to identify the introduction of any new substance and develop a descriptive picture not only of the individuals who use it but also of the social organization that supports its use. With regional stations, this can be achieved more quickly and accurately than with the present DAWN or CODAP systems, which depend on treatment agencies and treatment personnel to collect data.

Second, there is a need to understand the changing nature of the youth culture as it hops from drug to drug or develops unique interests, activities, and responses to the same national and world events that affect us all. For those of you who mingle on some regular basis with street people, it comes as no surprise that young drug users, even those who are committed to continued use, have interests other than drugs. They have long conversations about all those mundane things that occupy the interests of the people who study them or try to help them. We need to know as much about how young people view their world as we do about their drug use. Drug users do not exist in a world dedicated totally to drug use. They have opinions, attitudes, and personal

experiences about race relations, the draft, the oil shortage, confidence in government, elections, and so on.

In the past 10 years, drug experts seem to have become the key interpreters of youth behavior, and it is assumed that they have an intimacy and knowledge of all the current fads. They act, as it were, as one of the important mediators between the youth population and the "men who rule," most of whom have the same degree of sincerity as any of us in our concerns for young people who may or may not use drugs. I would suggest that ethnographic studies provide the kind of richness of detail, the texture of everyday activities that make what we call deviant behavior far more understandable, especially when we place it within the context of everyday life the way good ethnography does.

Third, I would like to make the kind of sweeping generalization that is both too simple and too facile but which, I believe, is nonetheless at the heart of the poor track record in the drug prevention field. Right now, among most youth, particularly those who use any of the illicit substances, there is a deep and pervasive distrust of government. In the drug field, this observation translates to mean that young people do not believe most of what the government tells them about drugs and narcotics, particularly if the message claims that the substance is harmful.

There is a need, therefore, to reestablish (and in some communities to establish for the first time) trust in government authority so that the vast resources of expertise that exist in the drug field can somehow filter down to those everyday users who may never get arrested or enter a treatment program but who run the risks of using illicit substances whose effects may indeed be harmful to them.

I am suggesting that ethnographers have a special gift of developing rapport with street people. And, when the ethnographers do their jobs well, they do not escape the notice of those local legitimate people--district court judges, community newspaper editors, members of the local chambers of commerce, the clergy, the parents of respondents--all of whom eventually come to trust the good intentions and special knowledge ethnographers develop as they study the local social structure. In short, an ethnographer who does his or her job well develops a local reputation long before any of the data have been analyzed and published. Usually, the trust that is necessary to gain access to information about criminal or drug-using behavior is easily transferred to other tasks. And one of those tasks may well be participating in local and regional planning. I do not believe that this has been given sufficient attention by either drug planners or ethnographers. It would seem axiomatic that findings and conclusions about drug users would be useful, supportive data in drawing up intervention plans. I would like to suggest that the knowledge an ethnographer develops about a community, particularly the informal local power structure, may be one of those crucial components so necessary to the successful implementation of plans. If the special knowledge of the ethnographer is used, it would be

possible to tailor intervention to the idiosyncratic nature of the local community and neighborhood, rather than to transplant modalities that may or may not be appropriate to local communities. If this could truly be accomplished, then the kind of success we hope for when we write those plans might have a better chance of becoming a reality.

In summary, I would like to say that this is a working symposium with two goals: to explore ways that ethnography and ethnographers can contribute to making State and local public policy through the development of plans for prevention, education, and treatment; and to consider plans for establishing ethnographic field stations.

We hope that by the end of the workshops, the important groundwork will have been completed so that we may move toward establishing the kind of collaborative effort between ethnographers and State policymakers and planners that will help make the strategies of intervention both more realistic and more human.

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State Problems and the Need for Research-Based Planning in the Drug Field

Richard J. Russo, M.S.P.H.

The committee asked me to identify problems or items for discussion that a State director must address in the process of developing a comprehensive State plan that ethnographic research might help resolve. We have identified several major issue areas for discussion that "cry" out for research. Whether or not ethnography can respond to these items is for you, the ethnographers, to decide.

DEFINING THE PROBLEM

All human services agencies, such as those directed toward alcohol and drug abuse prevention and intervention, are created as the result of a public definition of a problem, coupled with a concern for reducing or solving the problem. In particular, this concern must be felt and acted on by those who have the power to create institutions for the purpose of tackling the problem.

A problem can be defined as a recognized discrepancy between what exists and what is desirable. The greater the discrepancy, the more strongly we tend to view processes for reducing it as needs. Discrepancies can be reduced by changing what exists, by altering our view of what is desirable, or both.

For example, the increasing world population, coupled with a scarcity of food, has long been recognized as a problem. Means of addressing it depend on a variety of issues, such as the relative value placed on large families, religious sanctions against the use of certain foods, individual and culturally established tastes, and so on. Thus, we might determine that there is a need for careful family planning, or a need to increase productivity through agricultural research, or a need to change consumption patterns.

The problem of drug abuse in our society is complicated; the statement of the problem itself is central. It is clouded by history, customs, cultures, mores, and values. The problem cannot be seen in isolation--it is rooted in history, in culture, and in the nature of man.

An understanding of history is crucial to a definition of the problem. Here, the ethnohistorian is crucial. The study of other cultures is necessary for an understanding of how different populations define their world, how they behave in everyday life, and how they experience problems.

Also crucial to a definition of the problem is an analysis of our culture today. The difficulty with planning today is that it begins with public policies and public expectations that are not built on a sound analysis of our culture and its people. Policies are often developed during crisis situations, in the midst of a political process. They are developed with an eye to the expectations of the people and the expectations of political leaders. Therefore, policies are generally established with little consideration of either history, or past or present cultures.

As unclear as the definitional issue is, we in planning develop a statement of a perceived problem, and then specify needs--which leads us to engage in making value judgments. Not only are problems subject to change, but our definitions of needs related to them are also subject to change--sometimes almost independently of the actual situation originally defined as a problem.

In the drug field, the definitional issue is immediately translated to statements of the incidence and prevalence of abuse, which are most often examined through surveys and analyses of public domain records. Both provide important information for the planner, but both have serious limitations.

Surveys can be extremely expensive, particularly when they attempt to measure characteristics that occur infrequently within the population, thus requiring large sample sizes to produce acceptably reliable estimates. It is also difficult to obtain accurate information about drug use when investigators rely on self-report methods. Respondents are reluctant to report activities and behaviors that are illegal or regarded as private in their culture.

Analyses of public domain records, such as arrests, overdose deaths, illnesses related to abuse, treatment admission etc., are becoming quite sophisticated mathematically. Researchers in the field are using a wide range of multivariate techniques to analyze data from various sources. Again, however, these techniques have severe limitations. Reporting practices vary both geographically and over time. The biases in such records are often so complex as to defy rational explanation. Also, public records are inadequate sources to measure abuse patterns among the so-called "hidden" abusers. Such information does not come to public attention in a manner that allows reasonable estimates to be made about the extent of the problem. For example, it is only recently

that we have begun to adequately measure misuse of drugs among the elderly. So far, the data, obtained mostly through surveys, indicate that this is a major problem.

In New Jersey and many other States, we have used both survey and mathematical techniques to estimate the prevalence of heroin abuse. Although we found the results of both to be in general agreement, we recognize an additional common limitation. Both methods reduce highly complex behaviors to a single variable--extent of use. Qualitative aspects--such as clear distinctions of extent, frequency, and recency of use, and the relationships of these to physical, social, and psychological aspects of use--are either reduced to simplistic, quantifiable measures or ignored totally by these methods.

Typically, quantitative approaches, such as surveys and public domain record analyses, rest on a wide assortment of assumptions about the problem. Often, we tend to forget these assumptions--for example, the assumption that holds there is a clear and strong association between extent of use and the severity of the problem. That is, extent of use becomes the operational definition of the extent of the problem.

Qualitative research, on the other hand, provides a way of looking at more complex contexts in which the problem exists. It tends to focus on individuals and their relationships to their environment rather than on aggregates. Initially unstructured, ethnographic research provides a method of looking at the total situation; it makes few prejudgments, without a preestablished data collection plan, the purpose of which is to reveal the salient elements of the context in which abuse occurs. Such research certainly has a place in the drug field for providing insights into the nature of the problem, and can be an important first stage in the clarification of hypotheses, which then must be quantified for further examination.

In passing, I will mention that the Single State Agencies (SSAs) of New Jersey, Wisconsin, and Pennsylvania, as a consortium, are developing a National Prevention Evaluation Resource Network. As part of this effort, evaluation guidelines are being developed, and a section on the use of ethnography in evaluation research is being included in those guidelines.

ASSESSING THE MEANS OF SOLVING THE PROBLEM

Once having defined the problem, we must make determinations about what is needed to solve it. Such decisions rest in part on assumptions regarding the abuser. At one time, when the abuser was clearly labeled as "criminal," attempts were made to solve the problem through incarceration. As we moved toward relabeling the abuser as a "victim" or as "emotionally disturbed," methods

for attacking the problem changed. The history of the Public Health Services Hospital in Lexington, Kentucky, provides one good example of the interactions between relabeling and changes in our responses to the problem.

Through years of development, a set of treatment modalities has been devised--e.g., methadone maintenance, drug free, etc. In prevention another set of modalities has also been delineated--education, information, alternatives, and early intervention.

Given these treatment and prevention modalities, the planner must decide on how, when, where, and why they might be implemented. But there is simply not enough empirical information to make reasonably valid decisions. We do not have sufficient information on the efficacy of the various approaches. Limited resources favor those processes that have the fewest immediate requirements for expenditures. We are forced to use utilization rates as a major means of determining need. This method would be viable only if we had an assurance that a full range of reasonable alternative services is being offered.

In treatment we need research data to identify the present sociological causes of, or occasions for, drug abuse. We need more data on the effect of the treatment process on the family, the work place, the school, and the community. We need this research to help us to plan and develop models of treatment that are relevant to the total environment.

We need more data on the nature of the drug abuser and his or her individual needs. In the psychological approach that is based on the needs of self-esteem and self-image, we need to know more about the cultural relativism of our society and what values to build into a treatment program. For example, some of the concepts of "a community" in residential treatment are based on tribal concepts and customs that have been drawn from anthropology.

Prevention, the alternative approach, is referred to constantly, but alternatives cannot be developed without a clear understanding of our present culture and our present lifestyles. For example, we need help to plan alternative models based on peer group and family interaction. The disappearance of authoritarian norms has left a gap, and prevention planning should be based on insights into contemporary rites of passage to the adult world and on insights into the taboos that the young are told to avoid.

Special interest groups both purporting to represent the needs of the target population, and professing to know its problems, pose a planning issue that needs to be clarified through research. It is a twofold issue:

- Does the organization or its leadership really represent the special interest population?
- What kind of decisionmaking process exists between the advocate organization and the target population it purports to represent?

- Is it elitist, participatory, or both?
- What is the nature of the interest groups?
- Are the problems scientifically researched and identified, or based on feelings and biases of both?

SSAs are not familiar with the process through which certain special interest planning and programming becomes mandated by the Federal agencies and the Congress. To understand this process, examples of specific research questions may be: Do women universally require special treatment, or only a subcultural class of women--perhaps the white, middle-class, nonheroin drug-using group? Can this proposition be applied to the total class of white drug-using women? To the black and Hispanic women?

In other words, what were the justifications for mandating planning in special interest areas--facts, logic, felt needs, rhetoric, or a combination of these? What are the cultural and social characteristics that can deter or encourage the development process of a special interest program? This is not merely to ask what kind of prevention and intervention modalities are appropriate, but to identify special interest organizational capability and resources to plan and develop special interest programs.

These questions are equally applicable to the SSA decision-making process when SSAs are confronted by the special interest demands. To date, we have no methodology to research these substantive and organizational issues.

The result is that we who are involved in drug abuse needs assessment planning cannot depend on public data and surveys. We need to understand the customs, attitudes, and perceptions of the people we serve. We need researchers and data collectors who participate in the normal activities of people, and judge needs from that vantage point. This kind of research is especially important in identifying the characteristics, problems, and needs of special populations, including youth, the elderly, and minorities.

PRIORITIZING SERVICE DELIVERY

Unfortunately, program evaluation in the drug field is often based on utilization rates, coupled with the scantiest of evaluation research findings--heavily influenced, of course, by the socio-economic environment. Little use has been made of cost-effectiveness or cost-benefit analyses.

Cost-effectiveness analysis is relatively straightforward, assuming information on program inputs, process, and outcome has been well documented. Unfortunately, it is seldom that appropriate data collection takes place. The problem is further compounded by the fact that different service modalities have different effects

on different subsets of the abuser population. The question of cost benefit is clouded by such issues as assigning a dollar value to human life. Economists have developed almost as many approaches to this complex issue as there are drug abusers.

Relative costs and effectiveness measures can be major factors in the prioritization of delivery of services. Obviously, we want to use the most effective and efficient means possible of delivering services. This can be done only if a careful needs assessment has been made.

MOBILIZING COMMUNITY SUPPORT SYSTEMS

Community support encompasses the identification and mobilization of those forces that encourage community interagency authorities and resources to foster holistic drug abuse prevention and intervention. An overall strategy must seek to encourage the various community authorities and agencies to take a positive interagency approach to holistic drug abuse prevention and intervention.

In the past, the SSAs in following the Federal mandates and guidelines, focused their planning and utilization of resources upon heroin-oriented intervention services. It was both effective and appropriate for an SSA to assume the sole responsibility for managing and directing contracts and services to accomplish this task. However, this is no longer true. The focus has changed from heroin-specific abuse problems to polydrug, alcohol, and marijuana abuse problems; from drug-specific dysfunction to behavioral health dysfunctions; from treatment to prevention; from categorical to consolidated funding and planning; from relatively loose accountability of expenditures to cost-effective accountability; from the general population to women, youth, minorities, the elderly, and other special interest groups. Another factor beyond the control of the SSAs is the apparent public disinterest in supporting drug programs and the growing distaste for methadone maintenance.

The immediate issue is to identify a development process for "marketing" an interagency community support model. My question to this conference is whether ethnographic research can identify those characteristics in the human service systems in rural, suburban, and urban field stations, that will provide the basis for the effective mobilizing of community interagency support systems. Today, we have no structured methodologies for accomplishing this task.

STUDYING THE SOCIOPOLITICAL ENVIRONMENT

The common thread that runs through all of these planning considerations is the total environment in which the SSA operates.

The environment surrounding and affecting the agency is varied and complex. National and State policymakers, community support systems, clients, other social agencies, front-line service providers, project administrators, researchers, politicians, and a host of others have their own assumptions, biases, and attitudes--all of which affect the development and planning of the SSA.

Perhaps we need an "ethnographic field station" within each SSA to begin to study and address these crucial "sociopolitical forces."

In conclusion, I have identified five major problems or items that SSA planners must face. These items have not been adequately researched in the past. There are many more problems that could be identified that require "researching." However, in my estimation, those identified in this presentation are paramount. It appears to me that drug-related ethnographic research has been basically centered on heroin in the past--it must broaden out into the new areas I've identified above. I challenge you, the ethnographers, to look into the numerous challenging areas that have not been studied before.

What Is Ethnography?

James M. Walters, Ph.D.

Developing a universally acceptable definition of ethnography seems a challenge second only to developing such a definition of culture. I do not mind telling you that I sacrificed both sleep and the company of my friends as I pondered my assigned task. The problem, of course, is the diversity and methodological experimentation that characterizes ethnography today. It occurred to me that the most effective strategy was somehow to limit the scope of my presentation. So let me begin by telling you, in good pedagogical fashion, what I will not be discussing. I will not recite a catalogue of ethnographic techniques such as:

- Methods of collecting data that many ethnographers employ;
- Creative methods of handling data, such as the computerized methods with which Dr. Michael Agar is experimenting; nor
- Rigorous research designs such as representative case ethnography, which I employ in my own work.

Nor will I enterain you with a series of ethnographic anecdotes, which so often dominate our gatherings. Still, I would hardly be an ethnographer if I had no notes or anecdotes to share that illustrate what ethnography is, as well as the difficulty in explaining it.

All ethnographers, myself included, are asked unendingly to explain what we do. In my case, those questions often are asked by the staff and clients of local drug centers. As anyone who has spent time in such places knows, drop-in centers, crisis centers, etc. can be rather chaotic. So, I began portraying ethnographers as the ethologists of the social sciences. Just as ethologists learn more about animals in their natural habitat than in captivity, so too ethnographers learn more about people, in our case drug users, in their natural habitat--the streets--than in treatment centers.

In a more lighthearted vein, nothing bespeaks the frustration of explaining ethnography better than a story that another of our speakers, Ed Preble, tells.

After years of enduring the frustrating question, "What is ethnography?", and concocting scores of answers to suit the occasion, Ed was finally moved to learn the derivation of the word "ethnography." He consulted the Oxford English Dictionary and learned that ethnography is rooted in the ancient Greek word, "ethnos," which means, among other things, "heathen." Now, whenever Ed is asked the question, he answers, "I study heathens"; and that usually ends the conversation.

THE ROOTS OF ETHNOGRAPHY

Ed Preble's humorous riposte is not entirely facetious. First, ethnography often is considered the property of anthropology, and anthropology is viewed as synonymous with the study of savage, heathen, preliterate societies. Second, ethnography unquestionably is rooted in studying exotic people or cultures. For that reason, Herodotus is sometimes called the founding father of the method, but I will spare you that lecture. For our purposes, ethnography has roots in the fertile soil of American anthropology and sociology during the first quarter of this century.

At Columbia University, a physicist-turned-anthropologist, Franz Boas, was promoting fieldwork and instilling in his students two rigorous guidelines. Boas stressed that students of sociocultural behavior:

- Must abandon a priori assumptions about the group being studied and about aspects of their lives; and
- Must collect their data from reliable informants and report in the most reliable manner--namely the vernacular of their informants.

The value of Boas' relativistic ethnographic approach is amply demonstrated by the contributions his students made to psychosocial theory. For example, Edward Sapir's discussions of the relationship between language and culture; Ruth Benedict's studies of culture and personality; and Margaret Mead's life-long discussion of the pyschosocial dynamics of childhood and adolescence. Finally, though he was not a student of Boas, I think Bronislaw Malinowski must be acknowledged. His monumental ethnography of the Trobriand Islanders is stamped with the ideals of Boas. Malinowski probed the essential dynamics of the family and the psychobiology of culture. His work, together with Mead's, forced a greater flexibility into theories about the psychodynamics of sexuality and psychosocial development.

The enrichment that ethnography could bring to psychodynamics led many theorists to work closely with ethnographers. Among them were psychologists of great stature such as Harry Stack Sullivan, Karen Horney, Eric Fromm, Eric Erickson, and Abram Kardiner. Kardiner, I would like to note, conducted a long-lived

seminar at Columbia attended by many anthropologists over the years, including Ed Preble.

At about the same time that Boas was institutionalizing fieldwork among American anthropologists, Robert Ezra Park was joining with William Thomas and Ernest Burgess at Chicago to found what has come to be called the "Chicago School" of sociology. Like Boas and the East Coast anthropologists, Park and his colleagues valued conceptual theory but stressed its investigation and demonstration through participant observation in the field. Unlike the anthropologists, however, the Chicagoans' field was not an exotic island or tribe, but rather the city and its denizens. For us who have come to be called "street ethnographers," our roots are here, in the Chicago School of Parks, Everett C. Hughes, William Foote Whyte, Howard Becker, Erving Goffman, Gerald Suttles, Elliot Liebow, and others with their excellent reports on:

- The enculturation of medical students;
- The socialization of drug users;
- The sociodynamics of abnormality and mental illness;
- The complexities of life on the streets and in the slums; and
- The frustrations, adaptability, and industry of slum residents.

What marks all these works is their investigators' ability both to describe the characteristics and life-ways of their subjects objectively, and to allow those subjects to inform the reports with their own world view. That, incidentally, is one reason why ethnographers speak of informants rather than of research subjects. I must add, however, that you will hear fewer street ethnographers using the term "informant" for obvious reasons.

DEFINITION OF ETHNOGRAPHY

Ethnography is not just another method for capturing and massaging data. Like other methods in the social and behavioral sciences, ethnography is an analytic description of the behaviors that characterize and distinguish cultures or sociocultural groups. But unlike other approaches, ethnography goes beyond mere "objective analytic description" to include a description and analysis of the knowledge and beliefs that generate and interpret those behaviors. Ethnography's hallmark is this duality. The ethnographer "steps in and out of society," as Hortense Powdermaker put it. In other words, ethnographers describe the respondents and their world both as they see it and as the respondent sees it. Or, as Jennifer James said, ethnography is "the study of a culture from within."

Though they are usually buttressed by demographic and socioeconomic data, and sometimes by medical and psychosocial assessment tests as well, the two essential, indispensable tools of the ethnographer are:

- First-hand observations, especially participant observation whenever possible; and

- Formal and informal interviews with the respondent.

The ethnographer immerses himself or herself as fully as possible in the respondent's world, "hanging out" at the corners, arcades, luncheonettes, parks, school yards, and homes where respondents eat, sleep, work, and generally "take care of business."

In doing so, the ethnographer begins to understand the universe unique to the respondents, the social structure and values that organize their lives, and of course, the respondent's network of associates. The ethnographer and respondent thus build rapport with each other. The ethnographer demonstrates the trustworthiness and reliability so essential to evaluating responses, especially the more formal interview material. By gaining the respondents' trust, ethnography is able to elicit rich data often beyond the reach of other methods. Typical of the routine information street ethnographers obtain in their studies of drug users, for example, are:

- Range of drugs used;

- Ages and circumstances of initiation to drugs;

- Modes of administration;

- Techniques to assess and manage drugs;

- Economics of drug use;

- Drug effects;

- Social settings for drug use and their relationship to perceived effects of drugs;

- Physical and social benefits and risks associated with drugs;

- Adverse reactions and users' responses;

- Treatment histories and client assessments; and

- Criminal justice histories.

From these data, coded and sorted by topic, patterns of behavior emerge--in this case portraits of drug use. These patterns then can be associated not only with aggregate statistics on age, ethnicity, socioeconomics, and so on, but also with lifestyles and social values. Behaviors, including drug use, can be understood and evaluated in their real-world context. As Dan Waldorf will explain, this has been ethnography's greatest contribution to the study of drug use and other behaviors labeled as deviant.

STRENGTHS AND WEAKNESSES OF ETHNOGRAPHY

Critics of ethnography usually raise two doctrinaire objections to the method:

- Naturalistic studies lack the control necessary for rigorous hypothesis testing;

- Research, such as ethnography, that uses small sample sizes is not necessarily representative and, therefore, is not necessarily generalizable.

A priori, quantitative, controlled approaches do best demonstrate causal relationships, and are best able formally to test hypotheses. However, their strength, by definition, is dependent on critical variables being known in advance. But the tested variables often are chosen for some theoretical relevance and defined by the common assent of other "expert" researchers. Such an approach is really a two-edged sword. A priori methods cannot effectively rebut the phenomenological objection that increased control leads to an increased distortion of reality. Furthermore, the a priori variable may be both irrelevant, meaningless, or at least misunderstood, in the context of the real world.

A case in point is the confusion over the terms PCP and THC. All of us have read that PCP is regularly sold and bought as THC. The usual conclusion is that the drug user buying THC is being fooled. Someone is being fooled, but it may be the researchers. In my work I have learned that users generally know that PCP and street THC are essentially the same. Still, their names are not synonymous; they suggest different potencies. Are PCP and THC the same? No and yes. Misleading conclusions would result from a survey or clinical interview that asked merely whether phencyclidine was bought as PCP or THC. It would not even be enough to ask only whether PCP and THC were the same. This is precisely why it is impossible to construct an a priori ethnographic instrument.

If a posteriori approaches, like ethnography, are poor at demonstrating causality, they are best at detecting patterns; that is meaningful correlations in real life social contexts. In fact, in his article "The Mutual Relevance of Anthropology and Psychology," Donald T. Campbell notes that errors resulting from unsystematic sources in ethnography cannot rebut ethnographic correlations, because such error reduces rather than raises the level of correlation. In other words, ethnographers using reliable data are more likely to understate rather than overstate the strength of their findings. The key is high-quality, reliable data. In that same article, Campbell adds that three characteristics of ethnographers are particularly useful in insuring such high-quality data:

- The ethnographer's familiarity with the respondent's vernacular;
- The ethnographer's length of interaction; and
- The ethnographer's degree of participation in the respondent's world.

In short, there can be no substitution for the essentials: ongoing participant observation and interactions with respondents.

Ethnography is not an effective tool for formal hypothesis testing, which demands a rigor and control that the approach does not afford. However, probabilistic hypotheses have a way of evolving into dogmatic policies, as in the assertion that all drug users and addicts are psychosocially dysfunctional. Such statements are, themselves, informal hypotheses begging to be tested.

CONCLUSION

Ethnographers practice a difficult but rewarding craft that yields more than just gripping story lines. It gives a rich combination of theoretically valid, and administratively useful information about human behavior.

Effective social science needs a partnership of quantitative and qualitative approaches, each validating the other, each giving direction to the other's continued research. Ethnography has amply demonstrated that it is an effective partner in that enterprise.

A Brief History of Illicit-Drug Ethnographies

Dan Waldorf, M.A.

In general, most of our present knowledge about drug use and drug users emanates from research conducted in institutional settings--hospitals, prisons, and treatment programs--and is performed in conjunction with treatment or incarceration. These are settings where drug users go when they are in need of medical help, in trouble with the law, or unable to maintain themselves. Seldom will such institutions see drug users when they are "taking care of business" or otherwise managing their lives. Seldom will they see persons who are not experiencing some trouble in their lives. As a result, most of our knowledge of drug users comes from extreme cases--the PCP user who goes to emergency rooms for assistance; the barbiturate or heroin user who overdoses; or the drug user apprehended by the police--and in large measure does not apply to the majority of users or to less extreme cases.

This emphasis has caused some rather large gaps in our knowledge. For example, very little is known about controlled heroin users (chippers) because most of the research has been done among people who could not control their use and chippers can be hard to find. Still another example are untreated ex-opiate addicts. Until very recently they have been thought by some not to exist at all, but with only minimal effort the Recovery Project (of which I am co-principal investigator) has located 60 people who had been addicted to opium at least 1 year and have been "clean" for 2 or more years. Furthermore, we expect to locate 100 such cases by the end of the year. At present there is only a little information about the incidence (Waldorf and Biernack 1979) and not one study that describes these ex-addicts or the process of recovery.

Of course there are ways to find the unknown or the less visible drug user. Large national social surveys that are well administered do find them, but not without considerable effort and expense. Surveys of high-risk populations or groups find them also, but these surveys are almost as expensive as the national surveys. The cheapest way to reach the hard to find, less visible, less extreme cases is to move out of institutional settings and go into the street and use naturalistic methods. Of course not everyone can do this type of research. It requires training

and a certain style or temperament, but there are existing disciplines that train such persons. Unfortunately, naturalistic methods have not been used to study drug users with much regularity. This is changing, however, as I will show in the following short history.

BEGINNINGS

The first study to use naturalistic techniques with drug users was conducted by Alfred Lindesmith just after World War II and published in 1947. Lindesmith initially wanted to undertake a large survey of opiate addicts in Lexington Hospital or Fort Leavenworth, but when he went to the Public Health Service to ask for permission to do so, he was turned down with the explanation that they were doing similar research. His recourse was the street and he took to it. His first expedition into the streets of Chicago was with the assistance of one of the great characters of sociological research, Broadway Jones. Jones was Chic Conwell, the "professional thief" in the book of the same name by Edwin Sutherland, published in 1937. He had been addicted for a long period prior to Lindesmith's introduction to him and put the researcher in touch with several active addicts. Recently, I've learned that Sutherland was originally introduced to Jones via Herbert Blumer who liked to visit a doctor friend whose medical specialty was venereal disease and who treated a menage of prostitutes, pimps, petty gamblers, thieves, robbers, and safecrackers. Blumer enjoyed sitting in the doctor's office and talking to that throng of patients. I will say more about Blumer later.

Eventually, Lindesmith interviewed and observed 62 active addicts during a 2-year period. He usually conducted interviews over a drink or an inexpensive meal in bars and restaurants, but during the course of the study he got to know many of his informants very well, inviting some to his home.

Lindesmith was interested in the cognitive processes of opiate addiction. The idea for such a study came to him when he read in the literature that many people who were given opiates for prolonged periods as part of some medical treatment did not become addicted. In his own study he set out to test three different hypotheses to explore the processes of addiction. Eventually he concluded that people become addicted gradually when they take opiates to forestall withdrawal symptoms, and only after they make the cognitive association that opiates relieve withdrawal symptoms. This finding was a significant breakthrough in the understanding of the social-psychology of addiction (Lindesmith 1947).

The second naturalistic study was also conducted in Chicago, which is probably a mecca for field studies because it was the site for numerous studies. This study was conducted by Howard Becker in 1951. During an interview I had with Becker, he said

that at the time he had just graduated from the University of Chicago and was looking for academic work. He was working as a jazz pianist in a bar but wanted to put his degree to work also. He had an idea that he wanted to do a study similar to Lindesmith's with marijuana users utilizing the same method to find out if learning had anything to do with the marijuana experience. At a sociological conference he was introduced to Sol Korbin, by a friend and colleague, Henry McKay. Korbin was directing a study of juvenile drug users (called the Chicago Area Project) on a grant from the National Institute of Mental Health as part of the Institute of Juvenile Research. Becker told Korbin of his interest and Korbin put him to work for a year at $75 a week.

His research plan was to interview jazz musicians, whom he knew to be marijuana users, about the sequences of their marijuana use. He proceeded to observe and interview musicians at both his and their places of work, and gradually built up 50 interviews. At the conclusion of the study, he wrote the paper "Becoming a Marijuana User," which was a radical departure from the existing research of the time. He found that marijuana was a much less potent drug than the literature described, and the experience was so subtle that persons had to learn how to use the drug properly, how to recognize the effects, and how to enjoy them (Becker 1953).

During the course of the marijuana study, Becker interviewed the wife of his drummer (her pseudonym was Janet Clark) and finding her extremely articulate, he decided to conduct more interviews to find out about her life history as a heroin addict. He taped 10 or 12 such interviews; she proved to be particularly intelligent and insightful about heroin use, her own addiction, and the vicissitudes of "the life." Becker edited these interviews and gave them to Helen McGill Hughes, who edited them a second time. They are published as the book The Fantastic Lodge: The Autobiography of a Drug Addict (Hughes 1961). In my view this is the best first-person account of heroin addiction I've read (and I've read them all). It stands far above the writing of most researchers (myself included) and says more about women addicts than all the present research combined. Unfortunately, Janet Clark never saw the publication of her autobiography. She died of an overdose of barbiturates before it came out.

The Chicago Area Project was also responsible for a third study. This one was about black ghetto life and drug users and was conducted by Harold Finestone (1957). The report of this study became the basis for Richard Cloward and Lloyd Ohlin's (1960) theory of addiction, which described heroin users as double failures--people who could not succeed in either the straight world or the bent world, and who withdrew from life into heroin use. This theory is generally refuted by most field studies which followed Finestone's. From my own experience, I do not believe that Finestone's work is a valid description of drug users and does not deserve the attention it received in the 1960s.

STREETS OF NEW YORK

Toward the middle of the 1950s two New York City street workers (an anthropologist and a social worker) began to observe drug use among street gangs. Ed Preble, the anthropologist, who has been described as the grandfather of drug ethnographers, began to study heroin users in Yorkville in New York City. Preble's typical office is a storefront in a bombed-out building just waiting for the demolition team--it makes my ex-butcher shop storefront in San Francisco's Portrero Hill look luxurious. He has worked in Yorkville on and off for 11 years, with forays into East Harlem, Claremont in the Bronx, and the Lower East Side. His first experience with drug users was at a New York City Board of Education Community Center in Yorkville, where he worked between 1956-59. In 1959, he received his first grant to conduct field research on drug use among street gangs (again in Yorkville). From that project he went to a Lower East Side Program called Mobilization for Youth that provided funding for him to establish treatment and prevention services in a storefront. After that he worked in East Harlem and Claremont for 3 years (until 1968) on a large multicity study as part of the Notre Dame Study of Man.

Preble has a way with titles--for example "Taking Care of Business: The Heroin User's Life on the Street," (Preble and Casey 1969); or "Methadone, Wine and Welfare" (Preble and Miller 1977)--and a genius for getting to know drug users and describing them and their activities. In "Taking Care of Business...," which is probably the best summary of his street research up to 1968, he challenged the then predominant clinical and sociological theories that described heroin addicts as escapists or retreatists--dependent, passive, withdrawn, with inadequate personalities, who are unsuccessful in both legitimate and illegitimate domains. His long experience affirmed that addicts in the street do not fit those descriptions. Instead, they are "actually engaged in meaningful activities and relationships seven days a week" and their behavior is "anything but an escape from life" (Preble and Casey 1969). In short, Preble challenged the predominant myths of professionals who never saw addicts outside of treatment--people who saw addicts only during periods of crisis. Within the same article he presents a concise history of heroin use in New York City from World War I to 1968, describes the distribution network of heroin and the economic careers of addicts (how they hustle). It is a gem of a paper and is read and cited continually by a wide range of professionals in the field--physicians, pharmacologists, psychologists, sociologists, etc.

While Preble was working Yorkville, Harvey Feldman was hitting the streets of the Lower East Side in the Two Bridges Area, a working-class community between the Brooklyn and Manhattan Bridges. Feldman started out in the Hamilton-Madison Settlement House as a graduate student in social work at Columbia University in 1956 and remained in that community for 6 years as a street-gang worker. His first assignment was with the Junior Warriors, an adjunct to an older Italian fighting gang, who were less

dramatic but just as rough as their movie counterparts. They were adept at training social workers and were quite successful in running off many of the naive professionals who came downtown to "help" them.

They didn't run Feldman off because he was almost as street-wise as they were. He came back after his placement ended to work regularly among them. Heroin hit the gang in 1957, and Feldman spent the next 5 years elbow-to-elbow with heroin users on nearly a daily basis. He likes "to hang out," and this fit in well with the youths in his community. This close contact quickly disabused him of his earlier training (rather psychiatric and heavily Freudian) that described heroin users as isolated, friendless, dependent, and lonely.

At the time, Feldman was not particularly interested in writing up his experiences but was content to do his job as a neighborhood social worker. It was only later, when he went to Brandeis University to get his Ph.D., that he began to set down his experiences with drug users. While a graduate student he wrote several papers about his Lower East Side experiences, culminating with "Ideological Supports to Becoming and Remaining a Heroin Addict" (1968), which he wrote as a class assignment in Everett Hughes' course in "Occupations and Careers." Feldman is not so good with titles, but his content is excellent. His articles are tightly written and packed with ideas and concepts. Take his "stand-up cat," for example, an ideal type that he observed in the Lower East Side. In the street social system of that community the "stand-up cat" epitomizes the ideals of toughness, strength, and daring and is the high-status model for the young in that slum neighborhood. The young men who strove to become "stand-up cats" were the first to use heroin in the neighborhood and set the climate for the heroin use of others.

Succeeding neighborhood youths used heroin to gain status on the street. They used it to enhance their social position, not to escape their problems or retreat from life (Feldman 1968). Again, this challenged the psychiatric theory, which viewed pathology as the basis for heroin use. It also further explained the social basis for the use of heroin. This represented a clash between theory based on clinical interviews and psychological testing with a self-selected population and a theory based on direct observations in the setting where the behavior was actually taking place.

While he was still in graduate school, Feldman received a grant to study a group of Italian heroin users in what he calls Coastal City, a pseudonym for an area on the East Coast (which made me think of Boston); so he went back into the field. He met his first street contact (an ex-addict) in a local Italian restaurant. Shortly thereafter he set up a storefront and then moved into an apartment in the community. He worked in the community for 4 years and used the data for his Ph.D. thesis and three articles. Much of this work was built on his earlier interest in the social system of the working-class neighborhood (Feldman 1972, 1973). In the street system of East Highland he found that the youth

organized themselves into a ranking system of: crazy guys, tough guys, solid guys, jerks, and faggots. Crazy guys had the highest rank and jerks and faggots the lowest. The latter were prevented from using heroin because its use conferred status since it was considered to be risky behavior. Heroin use, along with fighting and verbal insults, was a means of ranking individuals in the neighborhood and was denied to those of low rank.

Some years later, while he was a fellow at the Drug Abuse Council, Feldman went back to his data and chronicled the progression of drugs in East Highland (Feldman 1974). Unlike the popular notion that youth begin with soft drugs and move to hard drugs--marijuana to cough syrup to heroin--he found that the progression was alcohol, cough medicine that contained codeine (called Tussar), barbiturates (usually Seconal), then heroin. Marijuana did not become widespread in the community until heroin was solidly entrenched and was used by youth who could not take the risk of heroin. Feldman noted that the interplay between public policy, which defined drug use as criminal, made drug use very attractive to the youth of East Highland because it was seen as potentially risky and exciting. When the initial use of cough syrups was given special attention by the police, drug use (and particularly heroin) was elevated to a higher order of risk taking. In short, youth were challenged to use drugs by all the attention drug use received from the police and other social control agencies.

ON THE WEST COAST

At approximately the same time as Preble and Feldman were conducting their studies on the East Coast, similar studies were being done in the San Francisco Bay Area at the Add Center Project funded by HEW. Herbert Blumer, one of the grand old men from the Chicago School, was director of the Add Center Project. The study was located in the School of Criminology of the University of California at Berkeley. The project began as a prevention effort in the flatlands of Oakland--a lower class area. The staff soon discovered that the project, as was conceived and organized, did little to prevent drug use. Initially they set out to identify a group of abstaining youthful ex-users and bring them together with current users for the purpose of controlling the use of the second group. By the eighth month of the project, they found that they could bring youths together but could not get them to give up drug use because users were "well anchored in their drug use and well-fortified in their beliefs against all of the 'dangers'. . . ."

At that point, project members decided they needed more information, so they took a research tack; and Alan Sutter, a graduate student at the School of Criminology at the time and an active member of the Add Center Project, made a major contribution to the project and ethnography with his systematic interviews and observations. Sutter set out to chronicle the process of how young persons become drug users and either go on to more

conventional lifestyles or continue as opiate addicts, hustlers, or violent criminals. He found that drug use was not the result of some pathological condition, but rather a complex social process that appeared natural to the participants. Furthermore, he discovered that drug use did not inevitably lead to the addict lifestyle but instead there were a number of alternatives--not the least of which was the conventional, straight style. Among youthful drug users he found four types: rowdies, potheads, mellow dudes, and players. Rowdies tended to become violent criminals; mellow dudes and potheads became straight and conventional; and players became street hustlers. Street addicts did not evolve from any particular type, but could come from any one of the four. In general, Sutter's work on the processes of drug use had far more basis in reality--because he went to the street to study people as they were--than the earlier theories that postulated psychological deficiencies or social stigmas (Sutter 1966, 1969, 1972; Blumer et al. 1967).

The Add Center Project also produced information about a speed (methamphetamine) scene in the Bay Area (Carey and Mandel 1968) and a college drug scene at Berkeley (Carey 1968). This research was conducted by Jerry Mandel and James Carey; it focused on gathering descriptive information on drug scenes about which little information existed. Their findings, like those of Sutter, have become common knowledge today, but for their times were iconoclastic.

OVER LAND AND SEA

Naturalistic studies are not the particular domain of anthropologists or sociologists, but are often used in other disciplines as well. For example, journalists use the technique regularly (most particularly Tom Wolfe and Hunter Thompson) as do physicians (Matt Dumott), epidemiologists, and some psychiatrists (Robert Lofton). The next major naturalistic effort among illicit drug users was conducted in Great Britain by a group of epidemiologists who set out to observe and plot the movement of drug "abuse" (heroin and methamphetamines) through a small city in England. De Alarcon and Rathod worked in a small hospital in Crawley, a new town on the southern perimeter of London. When heroin users began to come into their hospital for treatment (not particularly heroin maintenance as there was none at that hospital), they set out to study the rate and mode of heroin use among heroin users in the town. In the process, they plotted yearly incidence and sources of initiation (they called it contagion). They found that heroin "abuse" came in three waves. In the first, a small number of young people were initiated by persons outside the town. In the second stage, a small number of Crawley's youngsters initiated others in the town. From that group heroin spread, "exploding in the town," through the third stage. Two initial sources were found to be responsible, either directly or indirectly, for 32 and 16 heroin users when the epidemiologists plotted transmission trees. The majority of users (79 percent) were initiated directly

by other heroin users in the town (De Alarcon, 1969; De Alarcon and Rathod 1968; De Alarcon et al. 1969). Following De Alarcon's lead a group of researchers at the Addiction Research Unit, the largest drug research unit in England, conducted similar studies of heroin users in Cambridge over 3 years (Kosviner et al. 1968; Hitchins et al. 1971; Zacune et al. 1969).

BACK TO CHICAGO

Back in the United States, Patrick Hughes, a public health epidemiologist who did his initial work with addicts under Ed Preble's tutelage, went to Chicago to make imaginative use of a combination of ethnographic and epidemiologic techniques in a series of studies which described copping areas, the social structure of drug users and sellers, and intervention techniques used to get addicts to treatment.

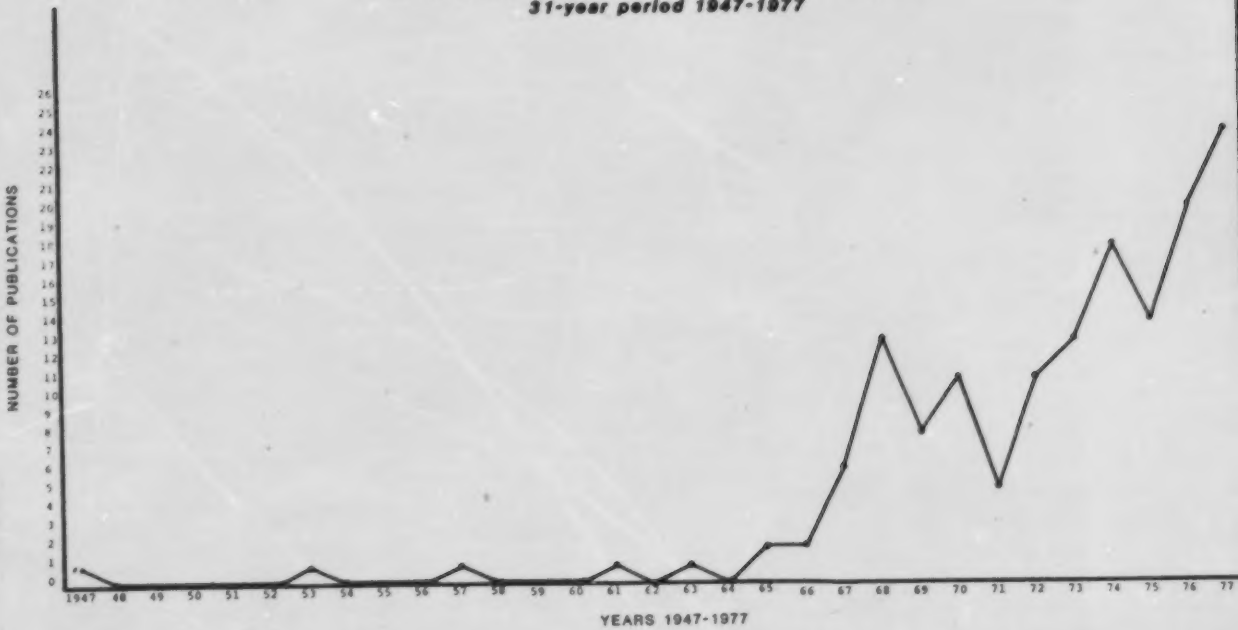
His technique is described in his book Behind the Wall of Respect. . . (Hughes 1977). He initially recruited four part-time nonusing addicts as field workers to gather information on addicts for the ostensible reason of helping them get into treatment. None of the four worked out; one became readdicted; another was hospitalized for surgery; the third and fourth became anxious about the street and the temptations it offered and quit the project. Then they found Rabbi--an older, methadone-maintained, former heroin dealer--who had high status in his neighborhood. He made immediate arrangements for Hughes, Noel Barker, and Gail Crawford to enter his community. Eventually, Rabbi gathered data on all the addicts in his community and then began to bring people in for treatment. Rabbi worked with the project for 2 years and then died of a pulmonary hemorrhage, which was not drug related.

The technique worked very well, and new information was generated about copping areas, the social structure of users and sellers (Hughes and Jaffee 1971), and the diffusion of heroin use (Crawford et al. 1977). They also found that naturalistic methods could be a useful tool for intervening in epidemics and getting people to treatment. (Hughes and Crawford 1972). We do not know what the long-term effects of these efforts were, but they were reported to be somewhat effective over the short run.

INTO THE 1970s

Since the 1960s, ethnographic studies have proliferated in both numbers and the directions taken. In the course of developing a bibliography of drug ethnographies, I plotted the number of publications for a 31-year period from 1947 to 1977 (figure 1). The first date, 1947, was that of the initial publication of Lindesmith's book Opiate Addiction. During the first 10-year period only two studies (Lindesmith's and Becker's) were

FIGURE 1.- *Number of publications that deal with naturalistic aspects of illicit drug users during the 31-year period 1947-1977*



published; in the second period there were 7; but in the third period there were 119. This is a sharp increase in the number of publications, but of course not every publication is a separate study, and many deal with methodologies. Nonetheless, the number of ethnographic publications has mushroomed, which would seem to indicate increased studies in the 1970s. I expect the trend will continue.

As for direction, there has been greater diversity as the numbers of ethnographers and studies increased. During the 1970s, there have been studies of:

- Chronic marijuana users in Jamaica by a group of anthropologists and physicians (Rubin and Comitas 1975);
- English drug users in Cheltenham (Plant 1974, 1975; Plant and Reeves 1976);
- East Los Angeles barrio addicts (Bullington 1977; Moore et al. 1974);
- Haight-Ashbury hippies (Cavan 1972);
- New Haven drug users and the treatment systems (Gould et al. 1974);
- Methadone users (Soloway 1974; Preble and Miller 1977);
- Speed users (Carey and Mandel 1968);
- Cocaine users (Cleckner 1976a,b, 1977; Waldorf et al. 1977);
- Long-term heroin chippers (most of whom were Washington bureaucrats) (Sackman 1976); and
- Addict-prostitutes (James 1976, 1977);

Of those studies listed above, the most important and far-reaching is the study Ganja in Jamaica (Rubin and Comitas 1975). This study was conducted with a staff of 45 professionals composed of anthropologists, physicians, psychiatrists, psychologists, and sociologists. It set out to test the amotivational syndrome and found that ganja, which has a higher THC content than most marijuana used in the United States, had no negative effects on work performance or intellectual functioning. This finding was not unexpected, because rural Jamaicans believe that ganja is generally beneficial and enhances their ability to work. Furthermore, the only negative effects were on lung functions and hematology, but this could have been the result of smoking tobacco rather than ganja. In general, the findings suggest that the effects of marijuana and ganja are, in large part, culturally determined, and that researchers should consider the cultural context of use when they wish to establish effects.

TODAY

At present, there are several ongoing ethnographic projects. Ed Preble and Bruce Johnson have a new storefront on 105th Street in New York's Yorkville district, there they are writing up their recent ethnos study and working on a study of the day-to-day economics of drug users. Harvey Feldman has just completed the report of a study of PCP users in four communities--Seattle, Chicago, Philadelphia, and Miami. James Inciardi is doing an ethnography of the criminal life of addicts and pickpockets. Marsha Rosenbaum is studying the careers of women addicts in San Francisco. At present, she has completed most of her interviewing and observations and is writing up her data. Joan Moore, Robert Garcia, and their associates are doing a study of two East Los Angeles Chicano street gangs that focuses on drugs and employment. They have recently published a book about their work titled Homeboys. Jennifer James is continuing her study of Seattle prostitutes, of which many are addicts. James Walters is conducting an in-depth study of suburban PCP users in Philadelphia, with the idea of operationalizing characteristics of youth groups so as to predict patterns of drug use. And Michael Agar, the author of Ripping and Running, has received a 5-year NIDA career award to develop quantitative methodologies for ethnographers.

SUMMARY

In general, the early ethnographers took the role of the iconoclast and played it very well. Seeing drug users on the street in their own communities, they quickly learned that most of the theories and findings of treatment-based research, which made up nearly all the research up to 1960 and a high percentage up to present day, was either erroneous or very much overstated. Consequently, a large part of their work challenged established myths, conventional wisdom, or denigrating theories that described addicts or drug users as being pathological or having personality deficiencies. In my opinion, they were correct in taking such a stance but assuming the role of the iconoclast always involves some pitfalls. By puncturing myths and attacking conventional wisdom, the iconoclast becomes subject to a lot of reflexive criticism from established groups who feel threatened by such attacks. I expect that ethnography among drug users in the 1950s and 1960s was slow to develop because it threatened established ideas and did not provide government agencies with the kinds of information they wanted to "wage war against drug abuse."

Since the 1960s, most of the work has been small-scale studies which are largely descriptive, perhaps too descriptive. Descriptions of social structures, drug scenes, and treatment programs abound with little concern for theory or theory development. At this point I think ethnographers should make a conscious effort to

move away from small-scale studies to create full-scale ethnographies on the order of the work of classic anthropologists, such as Bronislaw Malinowski and A.R. Radcliffe-Brown, and develop their own theories of addiction and drug use. I expect that only when definitive full-scale ethnographies are created and new theories developed will the field be given the kind of recognition and acceptance it deserves.

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Creative Tensions

Issues in Utilizing Ethnographic Research Within a Single State Agency

Bruce D. Johnson, Ph. D., and Douglas S. Lipton, Ph.D.

We wish to share with you the experience with ethnographic research at one Single State Agency (SSA). Our remarks will focus on three major areas: (1) the history of ethnography in the New York State drug agency; (2) the "creative tensions" that have arisen between an ethnographer (Ed Preble) and quantitative researchers (ourselves) as a means of exemplifying issues that may arise between ethnographers and SSA policymakers in the future; and (3) institutional factors that may limit the usefulness of ethnography to policymakers. In the discussion, we will try to convey both the rewards and potential difficulties of ethnography.

BRIEF HISTORY OF ETHNOGRAPHIC RESEARCH IN NEW YORK STATE

The New York SSA has been known under different names: Narcotic Addiction Control Commission, 1967-72; Drug Abuse Control Commission, 1973-75; Office of Drug Abuse Services, 1976-77; and the Division of Substance Abuse Services (1978 to present). During the preceding decade, this SSA has conducted applied and basic research, as well as evaluation studies, on drug use and abuse in a research bureau headed respectively by Daniel Glaser, Carl Chambers, and Douglas Lipton.

Prior to 1973, relatively little ethnographic research was conducted. When Douglas Lipton became the director of this bureau in 1973, he planned a research policy of "triangulation" to provide the kind of information needed to understand changing drug abuse patterns and to provide guidance for policymakers.

Triangulation holds that qualitatively different epidemiological approaches to the same phenomenon will yield a far more accurate and dynamic view. Each research approach complements the other's distinct strengths and weaknesses. Surveys of general and specific populations (secondary school students, college students) provide information about levels of use and abuse at a specific time, as well as statistical factors associated with drug use. Surveys, however, are costly, cannot constantly monitor changes, and provide relatively little understanding about associated behaviors of drug users. Unobtrusive indicators and other epidemiological data issued by other agencies in New York City and State may provide low-cost, long-term statistical data amenable to trend analysis and permit identification of changes in a rare-event phenomenon, such as heroin use. But no information is provided about why the changes (if any) occur, nor what kinds of people abuse drugs.

Ethnography and street anthropology can provide quality information about the social organization, thinking, and behavior patterns of drug abusers and the drug scene. It can also rapidly investigate pockets of drug abuse and provide early warnings about new drugs, combinations of drugs, and other emergent phenomena. But ethnography may neither provide information that is statistically valid for large populations nor document long-range trends.

Triangulation involves analyzing information from all three research approaches. Alternative hypotheses are tested against data, with the most reasonable set of interpretations provided to policymakers. Since 1976, the bureau has published fact sheets on trends in drug abuse that provide syntheses of quantitative data (New York State Division of Substance Abuse Services 1976-1978). In addition, the State plan also incorporates ethnographic findings (New York State Division of Substance Abuse Services 1977).

Mike Agar was originally hired to implement this research strategy as head of a sociocultural unit within the bureau. Several ethnographic projects were established under other project directors. Major efforts included:

- A study under Douglas Lipton and Don Vann of therapeutic and antitherapeutic informal group formation among clients within treatment facilities;

- A project under Paul Goldstein to constantly monitor the drug scene in 10 areas of New York City using trained ex-addicts who regularly visit key informants in those areas;

- A study under Jagna Sharff on the effect of drug use among Hispanic families in the Lower East Side; and

- A study by Paula Kraus of amphetamine users and diet-pill doctors.

Mike Agar also invited Ed Preble to submit a grant application to NIDA with him to study ethnic differences among white opiate addicts. The project was funded and began in 1975.

I became Douglas Lipton's assistant in July 1976 and assumed Mike Agar's role as the agency representative for Ed's project. I worked with Ed on quantifying ethnographic data in the life history transcripts, and on the final report of his study of Irish- and Italian-American opiate addicts. Ed's ability to obtain "hard-to-get" data from active street addicts was impressive. In addition, the report of the Panel on Drug Use and Criminal Behavior had just been released, and called for a systematic study of active addicts (Panel on Drug Abuse and Criminal Behavior 1976). Thus, the opportunity to research the links between drugs and crime was clear to Ed and me. We agreed to be co-principal investigators on a study of "The Economic Behavior of Street Level Opiate Addicts". This project, funded by NIDA in December 1977, combines ethnographic and quantitative research techniques to provide broader perspectives of the relationship between drug use and crime. A staff, with Paul Goldstein as Project Director, is currently interviewing opiate users, coding the data, and analyzing the social and economic behaviors of street addicts.

CREATIVE TENSIONS

This conference is designed to explore the potential uses of ethnography for aiding policymakers. If this potential is to be realized, both ethnographers and policymakers will be likely to engage in a dialogue that will involve a "creative tension." Such communication will involve tension due to differences in perspective, ideology, training, pressures from other constituencies, and relative closeness to the problem. This dialogue may also be mutually creative as policymakers attempt to grapple with ethnographers' reports of drug use and as ethnographers learn more about the constraints and demands placed upon policymakers by other sources.

Working closely with Ed Preble on a major research project has provided us with the opportunity to experience many "creative tensions" that arise between an ethnographer and a quantitative researcher. Many of our experiences appear to be part of the creative process that may emerge between ethnographers and policymakers. In the following discussion, illustrations from our research will exemplify a point, not resolve a substantive issue.

THE ISSUE OF GENERALIZABILITY

A major creative tension revolves around the generalizability of ethnographic finding(s). Ethnography is labor intensive. An experienced practitioner can generally focus on a few particular cases or in a specific area. On the other hand, policymakers and agency directors are expected to develop responses for diverse social groups in many geographical areas. Ethnographic findings

that are true in one area may not be the same elsewhere. In late 1977, the New York State Division of Substance Abuse Services was concerned, as was NIDA, about the use of PCP (angel dust) among American youth. A triangulation on the problem was begun. Ed was asked to find out what he could about PCP use. Based on ethnographic research in East Harlem, Ed concluded that phencyclidine was not a major drug of misuse among street opiate addicts, nor did it seem to be an important drug among East Harlem youth. (One old-time addict reported that PCP was some kind of "embalming fluid" sprayed on marijuana.)

Other evidence, however, indicated a contrary conclusion for other parts of the State. A statewide survey of drug use among secondary school students had been planned and PCP was included as a separate drug. Negotiations with drug program directors and school district officials around New York City and State were underway in late 1977. PCP was relatively widespread in several districts, although there was variation from district to district. In addition, reports received from the Drug Enforcement Adminstration in New York City indicated that PCP frequently was being sold in Central Harlem as a substitute for heroin or in conjunction with heroin. When the results from the statewide survey became available in November 1978 (New York State Division of Substance Abuse Services 1978), PCP use was much higher than originally anticipated: 15.5 percent of the students in grades 7 through 12 had used PCP at least once during their lifetime; 9.6 percent had used it since school began in September 1977; and 5 percent had used it in the past 30 days. After marijuana and hashish, PCP was one of the most common substances used by students. Moreover, PCP use in New York City and Long Island was higher than in other areas of the State. This example suggests that policymakers need to be aware that ethnographic findings cannot be generalized to broader populations without other corroborating material. Ethnographers, who are well aware of this limitation, need to develop techniques for improving the generalizability of their findings.

CLARIFYING CHANGE

The other side of the generalizability issue, however, is that ethnographers can provide new insights and clarification about important changes documented through statistical and epidemiological techniques. Data from a variety of survey studies and reports indicate a decline in heroin addiction and initiation into heroin use, particularly among black youth (O'Donnell et al. 1976; Johnson 1977; Boyle and Brunswick 1978). As part of the triangulation approach, the Division of Substance Abuse Services has been tracing epidemiological heroin indication--such as opiate-related arrests, serum hepatitis, and entry into treatment and detoxification programs (Division of Substance Abuse Services, 1978; Frank et al. 1978). These indicators showed a decline in 1977 and 1978. Ed has also encountered difficulty in locating "new" heroin addicts in East Harlem. "New" heroin addicts are

defined as people using heroin daily for the past 30 days but who began using heroin in the past year.

Ed recently recruited a long-time drug dealer as a respondent. "Brownie" deals heroin and cocaine in his tenement apartment; this is called a "house connection." During the initial interview at Brownie's apartment, Ed observed four or five blacks in their late teens and early twenties enter, go to a backroom where they bought and occasionally injected heroin or cocaine, and then leave. Brownie stated that most of his customers were similar to these people. But Brownie reported a big difference today from a decade ago. Most of these young heroin users visit two to three times per week or less. They use heroin on an irregular basis, much as they use cocaine or other drugs. Ed now refers to this type of heroin user as a "new breed." They are careful to limit their heroin consumption, generally cultivate a neat and cool appearance, and strive to avoid being seen as a greasy "junkie." In many respects, these users exhibit many of the traits of the "cool" drug user pictured by Finestone (1964). Thus, current social norms among ghetto youth appear to proscribe heroin addiction, although irregular heroin consumption is tolerated, as is the consumption of other substances.

Thus ethnography can provide critical insights about current social norms and behavior patterns among potential addicts. Further, these ethnographic findings converge or "triangulate" with statistical data from surveys and unobtrusive epidemiological indicators to provide a fuller understanding of how the addiction scene is changing. The division's director has begun shifting resources to deal with the widespread and expanding use of other substances among high school and young adult populations and to deal with prescription drug misuse among the middle-aged population (Martinez 1979).

PURSUING THE TANGENTIAL

To the quantitative researcher and the policymaker, a distracting feature of ethnography as well as one of its major strengths, may be called the pursuit of the tangential. Quantitative researchers generally investigate a limited set of questions, concepts, or hypotheses that sometimes are too narrowly focused. Policymakers frequently need brief, relevant memoranda or summaries of findings that are immediately relevant to policy to make decisions on a specific issue. But ethnographers, while pursuing a set of goals or objectives, also learn a great deal about topics that are marginally or tangentially related to his objectives. Frequently, other long-standing interests may be researched in the relatively unstructured field situation.

During the course of our relationship, I have become convinced that Ed is one of the nation's leading experts on the following topics, all tangentially related to his research on addicts: shoplifting, fencing of stolen merchandise in ghetto neighborhoods,

ethnic conflict among prison inmates, social history of the ghetto Italian and Italian lower class, and patterns of professional and semiprofessional crime. Ask Ed to expound on any topic and he will provide hours of fascinating insight. With a few more hours, he can provide quotable materials from previous interviews and field notes to back up his observations. For a policymaker, such breadth of experience, combined with an ability to rapidly investigate almost any issue, makes an ethnographer a valuable resource--especially when little is known about a topic.

THE LURE OF THE STREET AND THE PAIN OF THE PEN

From the administrator's viewpoint, a major problem with almost all research and evaluation is the long timelag between a project's initiation and completion. Policymakers frequently need a reply tomorrow, and may be impatient with the slow pace of research or with products that appear to be too technical or too obscure to meet their needs. In this regard, ethnography can generally provide more rapid, less technical results than can quantitative research. Nevertheless, ethnography involves time-consuming activities that policymakers need to appreciate.

The "lure of the street" or the research site provides a major source of psychological well-being and satisfaction for the ethnographer. Research subjects and/or location, by ethnographic standards, must be studied in their complexity. Although focal objectives may guide research, important and interesting tangential information may arise. Further, because subjects "fit" the ethnographer into their lives, the latter must make himself available when subjects engage in the activity of interest. A 9-to-5 workday can seldom be followed. As Ed will discuss later, the ethnographer needs to show a human interest in each respondent as a person, and not just as a research subject. Any spare time must be spent writing field notes, keeping records, and getting ideas and observations on paper. All of this takes time, a lot of it!

The "pain of the pen" refers to difficulties of ethnographic analysis that are seldom apparent to an outsider. Facts and numbers do not leap out of field notes. Conclusions emerge after frequent rereadings of field materials. Observational evidence and tape recorded conversations may be contradictory, insufficient, vague, or irrelevant to a particular point--but are likely to bear upon it in some important respect. Even when a conclusion seems firm, assembling the field materials and incorporating them into a smooth and logical exposition may be extremely difficult.

THE EMOTIONAL IMPACT

In some respects, ethnographers are journalists in anthropological clothing. They judiciously use quotations, anecdotes, and observations to tell a story that supports a major conclusion they wish to

present. As journalists learn in school, the lead sentence or paragraph has to grab the reader and induce him to continue. A story "grabs" interest when it appeals to emotions in a vivid and dramatic manner while downplaying "facts." The dramatic element in ethnographic research may serve a valid attention-getting role that may stimulate action. The policymaker can use ethnographic materials in public presentations and speeches to generate interest, add flavor to dry reports, and illustrate points better than numbers often do.

INSTITUTIONAL FACTORS LIMITING THE IMPACT OF ETHNOGRAPHY

The results of ethnographic research on drug use only occasionally find their way into agency policy. Ethnographic materials have had a more limited impact than data from quantitative surveys of the population or the evaluation of drug treatment. This conference may help reverse this situation and help ethnography more fully realize its potential.

Several institutional obstacles to using ethnography in an SSA will be identified, and an example given of how ethnography can be effectively used in policymaking. Trained ethnographers are rare. Ethnography has grown out of the cultural anthropological tradition or participant-observation activities of sociologists--both have few practitioners. This room contains many, if not most, of the active ethnographers who have done significant work in the drug field. The shortage of trained ethnographers suggests that a State agency interested in ethnography may have difficulty locating a trained person.

At this time, for instance, NIDA and Law Enforcement Assistance Administration (LEAA) research grants rather than State funds are supplying the salaries of Ed and his field staff. Other ethnographically trained staff are still on State salaries, but their main concentration is in treatment followup. They only pursue ethnographic data on an as-needed basis.

Most ethnographers, as well as other researchers in the drug field, are generally outside of the State agency. Moreover, ethnographers, as well as most academic researchers, are seldom a part of the political process with which policymakers deal on a daily basis. That is, ethnographers are seldom in touch with the constituencies and pressure groups lobbying for funds or particular policies. Thus, ethnographers also have an opportunity to communicate with many diverse groups other than government administrators.

Ethnographers appear to be highly self-selected; they enjoy talking and dealing with people in their natural settings. They tend to value independence. Ethnographers also are trained to explain the behavior of respondents using the subjects' own

concepts and definitions, as well as their observations about the effects that institutions and agencies may have upon respondents--all of which may be interpreted differently by policymakers.

Ethnographers do not fit comfortably into civil service practices. They exhibit unconventional modes of appearance by wearing street clothes rather than a tie and suit. They have variable work hours that follow street time rather than office hours. They write at home rather than at the office. They use cash for respondent payments and field expenses but obtain no signed receipts. The policymaker needs to appreciate this need for flexibility and assume that the ethnographer is honest, doing his job eagerly, and obtaining results that may be useful in the future.

Ethnography and what will be called "quasi-ethnography" can directly respond to and aid the policymaker. For example, when the new commissioner of the Division of Substance Abuse Services, Julio Martinez, had just been appointed, he was asked by a city councilman to find out about a potentially dangerous unknown substance. Douglas Lipton received a call about 10 a.m. on the Friday before Christmas. He was asked to get the field staff busy and provide a written memo about this substance within a week. This substance was none other than "Loony Balloony". Yes, we asked the same questions you are asking: "What's that?" It is a tube of plastic cement, the contents of which are placed at the end of a straw, then blown up into a balloon. But children were believed to be inhaling its acetone fumes, and we were asked to determine if a new problem might be brewing.

A phone call to our storefront sent Tom Miller, an experienced fieldworker, in search of "Loony Balloony". At noon, he called back; he had purchased tubes at a nearby candy store and also bought a similar product, "Balloons." The candy store owner claimed to sell about 100 tubes per month. Further research in the following week, involving conversations with children, youth, neighborhood residents, and known heroin users revealed almost no knowledge or abuse of this product. In addition, other former drug users on our research staff living in different parts of the city also found little knowledge or abuse of "Loony Balloony". In addition, research staff called directors of drug treatment programs, school program counselors, and counselors of other problem youth. Program staff may be considered as "quasi-ethnographers," because they are likely to come into contact with abusers, have a great deal of tangential knowledge about many subjects, and be much more aware than the average person of particular substances being abused within their local area. Although such quasi-ethnography may be less satisfying than a report from an ethnographer, the advantage of rapid response from a diverse set of communities will provide enough information to indicate whether more action is needed or not. Our conclusion, forwarded to the director, was that "Loony Balloony" was not a product to worry about.

The ethnographic research of Ed and his fieldworkers, combined with reports from program directors and others, however, documented the widespread use of other inhalants. The statewide school survey also indicated that inhalant abuse was very widespread among junior high school youth, less so among high school students (New York State Division of Substance Abuse Services 1978). Thus, a specific research effort is now being directed towards ascertaining what substances are being inhaled and the dimensions of such abuse.

In conclusion, although institutional obstacles and creative tensions may emerge, ethnographers can aid policymakers in many ways--limited mainly by the communication links between them and the questions asked by both parties. When confronted by an issue, a policymaker can easily phone one or more ethnographers (from in-state and out-of-state) to obtain opinions, observations, and input about almost any topic. Because ethnographers obtain so much tangential information in their work, they may be able to offer refreshing new insights about almost any issue. Further, the policymaker may request a short memorandum providing a more formal response to the issue, including further preliminary research on the topic. If more intensive research is indicated, the ethnographer can so indicate; of course, the policymaker may also seek information from other sources.

In addition, policymakers should consider spending a few hours two or three times per year on the streets with an ethnographer, talking with drug abusers, addicts, and experiencing the emotional impact of the street drug scene. Questions and observations will spark conversations about potential policy options and facts about the street.

On the other side of this communication link, ethnographers encountering a finding or emergent phenomena that may be important to a policymaker or administrator should feel free to contact the agency director. Usually, the director can be reached directly, or a staff member will be asked to handle the call and provide the administrator with a summary--which may also be requested from the ethnographer. Likewise, an invitation by the ethnographer to a policymaker to visit the streets will probably be welcomed.

Many other possible relationships between ethnographers and policymakers may emerge from this symposium. Establishing communication links where no or little interaction currently exists is an important topic for discussion.

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Research in Minority Communities

Collaborative and Street Ethnography Models Compared

Joan W. Moore, Robert Garcia, and Ramon Salcido

East Los Angeles is a vast tract of Chicano neighborhoods lying just east of downtown Los Angeles. In terms of drug research, East Los Angeles is remote, sensitive, and hostile--the natural consequence of two generations of police harassment, particularly of the youth gangs. It is important in drug marketing and the inhabitants suffer severely from a high rate of drug usage and the consequent incarceration. Recent estimates show that about one-fifth of the Chicano families in two barrios are affected directly by the criminal justice system (Moore et al. 1978).

In this environment, the Chicano Pinto Research Project has been successful with a collaborative form of research (Moore 1977). These studies rest on the active participation of a group of Chicano ex-offenders and ex-addicts with professional social scientists, paralleling the work of other researchers in the early 1970s, who were concerned with accountability (New et al. 1972). Originally, we hoped to describe this collaboration in standard ethnographic manner, drawing on the patterns of street ethnography (Weppner 1977), but the divergences are so sharp, the methods so different, that we have chosen instead to focus this paper on a comparison with standard ethnographic procedures.

COLLABORATIVE METHODOLOGY

The Chicano Pinto Research Project is a nonprofit corporation in East Los Angeles that is organized to do research, educational, and demonstration projects relevant to the problems of Chicanos with the criminal justice system. The word "pinto" means convict in Chicano slang, and most Chicano pintos are involved with drugs--primarily heroin. Most are also involved with the barrio gangs, a significant and semi-institutionalized feature of many

Chicano barrios or neighborhoods (Moore et al. 1978). Our board of directors includes three social scientists, directors of two Chicano drug abuse agencies, one former director of a gang project, and one pinto; our institutional review board is chaired by the director of a third Chicano drug abuse agency and includes the directors of major health and mental health community-based agencies in East Los Angeles.

At this moment we are doing research funded by the Department of Labor (DOL) and the National Institute on Drug Abuse (NIDA).[1] This means that we have an office, a staff of four full-time and two half-time pinto researchers, a pinto administrator-researcher (Robert Garcia), several clerical workers, and two professional social scientists (Joan W. Moore, a sociologist, and Ramon Salcido, a professor of social work). We have been fortunate in obtaining some CETA workers. Our research is focused on two barrios that have had youth gangs for more than 35 years. These gangs are age graded, and we are interviewing (using a quota sample) two members of each cohort from the very earliest to the present. Each cohort is named--e.g., "Tinies," "Monsters,"--and is reasonably definable in terms of membership. Our staff includes researchers who are themselves members of each of the barrio gangs--one older and one younger--and are also ex-offenders. We have been attempting to develop a barrio-specific research staff that is balanced in terms of sex and age, and also to encourage particularly talented pintos not from our target barrios. In addition to the quota for members of gang cohorts, we have set a quota for current occupational and drug status, ranging from the "hard core" (still using drugs uninterruptedly and in and out of jail and the California Rehabilitation Center) to men who have consolidated a substantial occupational mobility and have been drug-free for many years.

Our primary research instrument is a structured interview guide, which includes a detailed life-history section, and a section on the characteristics and behaviors of the respondent's cohort. These interviews are taped and transcribed, and the community research staff works with the professionals in delineating and interpreting data patterns.

THE NATURAL HISTORY OF THE CHICANO PINTO RESEARCH PROJECT

Although the development of the Pinto Project is unique, we believe that the roles and the division of labor that have emerged are generalizable, and that the model can be used in other minority communities with a history of convict-addict self-help concern.

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The first phase of our history began in the late 1960s. Garcia was working in an East Los Angeles shop, and Salcido was directing a community organization. Moore had received a 3-year Special Research Fellowship from the National Institute on Child Health and Human Development (NICHHD) and was concerned with providing the data from a recently completed study of Mexican-Americans to community groups. One of the groups that found this offer interesting was the League of United Citizens to Help Addicts (LUCHA). LUCHA had begun inside the California prison system as a self-help group, and was engaged in producing a "Peoples Resolution," a massive document that would substantiate the need for a ballot initiative to change California's repressive narcotics laws. The group had a very small membership, but its leaders had strong support from other convict self-help groups in and out of the prisons; its board of directors included a wide range of mature people, influential in the community, who were concerned that the pinto issue receivé an appropriate place in the swelling Chicano movement (Moore 1972).

Some of LUCHA's operating principles were built into a Model Cities program. Moore chaired the board of directors and, after a collapse of administration in 1973, directed the agency for a summer. At this point the second phase began. During this summer, discussions began with Gladys Handy of the National Science Foundation (NSF) and Eleanor Carroll of NIDA to engage in some structured research. Several of the pinto staff of the agency worked with Moore in developing some initial concepts. Funding began in 1974-75. The NSF study emphasized adaptations after prison. The NIDA study emphasized drugs. This dual funding made it possible for us to "get back" to the reality--that is, drugs and prison are closely linked in the Chicano barrios. We focused the study on three barrios, each a target area for an existing program. A latent intent of the study was to provide data for enriching these programs. We studied four types of social phenomena that usually escape urban history almost entirely. These are the growth of inner-city barrios, gangs, the drug market, and the continuities between barrio and prison--especially as they are expressed in the prison self-help groups.

Chicano ex-offenders and ex-addicts were active collaborators in this NIDA/NSF study and, in fact, are the four contributing authors in the Homeboys book (Moore et al. 1977). (The history of this phase of the Project is detailed in an appendix of the book.)

This work ended in 1975. Two monographs and a book were the direct result. But more importantly, the indigenous researchers had gained a reputation in the pinto community as a research group that was serving the community. At this point, we realized that the group would need a permanent form.

The third phase (late 1975--early 1978) came when the group was forced back on its own resources. We were incorporated as a nonprofit research group and the director, Robert Garcia, continued the organization without funding, and yet took an active

role in East Los Angeles organizations concerning ex-offender affairs. We wrote proposals, we advised--at that time we were almost entirely a voluntary community-based organization.

The fourth phase began when we received funds from the Department of Labor and NIDA. We rapidly built the core staff back into a functioning group. Only three of the old group were still actively associated with the project. When we traced the other former staff members we found only one person working in a steady and remunerative job. Virtually all of them had undergone major family problems and the ex-offenders had major employment problems.

Yet this problem of dispersal (most of it forced on the staff members by employers who simply will not accept ex-addicts or ex-offenders) was less important than our corporate inexperience. We had no negotiated overhead and had not written operating costs into the NIDA proposal. We submitted a supplementary proposal. Meanwhile, we tried to get started with volunteers, donated space, and donated equipment. We found all three--this is some indication of the strength of our community support. But there are important problems for new organizations in collaborative research. The community accepted us and we were beginning to find some receptivity in the academic world, but we were still new to Federal funding agencies. Our administrative money was inadequate and our budget was unrealistic, providing no money for an accountant. The budget item for an administrative assistant was stricken and we had no money for insurance. In time, mainly with the tolerance of a great many community people, we were able to begin operations. We experienced other peculiar barrio problems, such as inability to insure our staff because they are all high-risk health costs; the simple matter of auto liability insurance created a staff crisis.

I wish to stress this important point: Not only is collaborative research somewhat new in social science methodology, but there are important structural obstacles in Federal funding.

The book, Homeboys, based on the 1974 and 1975 project appeared this year and boosted morale. It received good reviews in the local press and strangely enough, this validation by the outside world was important. For the first time, researchers had entered the community and actually produced something that the people could see. This, too, must be emphasized. Federal reports may be meaningful to the career of a research firm, but the community sees no practical change in the condition of their lives. They are completely uninterested in contributing to knowledge--especially when they suspect, as do we all, that the contribution to knowledge is either negligible or will either be used against them in another administrative rule or ignored. A researcher's assumption that a subject is too remote from the mainstream of academia or government policy to really understand the ultimate use of the interview is noticed immediately by the respondent.

RESEARCH MODELS AND DRUGS

There are at least four contexts within which the researcher and the drug user find each other and interact. Each context involves a different user role and status set. Thus, each context has its own special constraints and opportunities for the researcher and also for those who are concerned with treatment implications. The following taxonomy--like all other ideal analyses--exaggerates, omits, and selectively distorts for heuristic purposes. It is designed to highlight the features of the collaborative research done by the Pinto Project. References throughout are predominantly to Chicanos and to East Los Angeles.

INSTITUTIONAL AGENCIES

Some users, especially young boys and girls who are involved with gangs, become involved with the criminal justice system--police, probation, juvenile hall, probation camps, and Youth Authority facilities. Older heroin addicts are also involved in the "revolving door" relationships with the county jail and the California Rehabilitation Center (CRC), in addition to prisons. Of course, the use-context for each type of user is different. Young Chicanos use drugs primarily in a wider range of settings that tend to be more private. Institutional contacts may lead to information about such settings, but this is rare. Usually there is only question-and-answer information.

Research inside institutions is constrained because the status-set of the subject revolves around his or her inmate role--present, past, or future. Such research includes studies of probationers, CRC inmates, prisoners, parolees, inmates of Lexington or Fort Worth, and jail inmates. There is considerble variation in the research opportunities and constraints presented in each of these specific settings, and there is considerable variation in the creativity of individual researchers. Michael Agar's work (1973) in Lexington, for example, appears highly creative in a prison context that presents a research climate less unfavorable than that in other prisons. It is obvious that Agar would have found it almost impossible to do similar research in San Quentin. Some nice people have done boring work in Lexington. All research suffers to some extent from the obvious constraints imposed on both the researcher and the respondent by the institutional setting.

Treatment implications taken from research in institutional settings context are usually congruent with the incapacitation, punishment, deterrence, and institutional-treatment models. The penal model has provided most of the treatment for our population of heroin addicts. That is to say, more addicts have probably spent time in prison, jail, or in CRC-type settings than in "treatment." (We are gathering some data now that are relevant to this question.) Studies done with correctional clients either wind up generally suggesting improvements in the institutions or critical of the correctional services.

COMMUNITY AGENCIES

A larger number of users become involved with community agencies. Although currently not the subject of official concern, they are involved in some kind of intervention effort and are not yet ready to move away from this context. They may, or may not, have had correctional experience.

In providing research contacts, the community agencies still focus on the client status set, although this is broader than the client status set of the prison or the probation camp. Research based on such community agency contacts is methodologically very diverse. It includes studies based on the CODAP files and also more qualitative studies, such as the one by Bullington (1977) of the Narcotics Prevention Project of East Los Angeles, Geis and associates of paraprofessionals in the same agency and of a local halfway house. The treatment implications of research in this second context tend either to be critical of the kind of treatment being offered or follow the individualistic health or mental health model followed by the particular agency. Evaluation research often falls into this category.

STREET CONTEXT

Research contacts for street users may be initiated at any one of a number of points, including the institutional and community agencies. But prototypically they are formed in the relatively public places where "street" users may be congregating. Usually this is a context for use as well as for congregation, in which age-peers use drugs in relatively public places. The focal status is that of "deviant" in most cases, and the particular version of deviance may be full-fledged (the criminal activities of heroin addicts) or partial (a group of young men blowing grass behind a bar). At either end of the "deviance" spectrum, the status set usually emphasizes deviant roles.

This research model, of course, is street ethnography. It depends on the personal contacts of individual ethnographers and the skill and concern of individual ethnographers in joining user networks. In such research there tends to be an emphasis on discovering lifestyles and norms within a particular set of people. It is almost a scaled-down version of an earlier concern with "culture."

The model has no implications for treatment because the research contacts are usually made in a segregated role set--that of use context. Yet there are some implications that might be drawn. It might be possible to use such an approach to tap into the natural networks for potential service delivery, as has been the outreach approach in local high schools that makes ex-addict counselors available.

COLLABORATIVE MODEL

The research contact points in the collaborative model are embedded in routine interactions between barrio homeboys. Thus, although this model emphasizes the gang-drug-prison status set, it also necessarily involves the kin and fictive kin (compadrazgo) status sets; and thereby potentially taps into every drug use context in the barrio. And, for that matter, outside the barrio as well because relatives move to various parts of the city. Thus we hear about homebound old people, middle-aged square women, pill-poppers, undocumented workers, husband-wife couples, in addition to the street drug users of model 3, and the clients of models 2 and 1. (Incidentally, it appears to us that there is a substantial male bias in the other three models, considering differential patterns of arrest, incarceration, utilization of agency services, and--especially of Chicanos--of "appearance on the street.")

The treatment implications of this model are substantial. The researcher observes or hears about systems that can give direction to services and networks that can translate unmet needs into service contacts. It is inherently an action point of view, incorporating and relating to community resources and viewing the development of the individual and the development of the community as linked. The collaborative approach (like the New and Hessler commune) involves other community agencies. This permits--although obviously, it does not guarantee--a mobilization to deal with emergent needs relating to drug use and also to needs apart from the drug network--such as jobs, birth control, and such needs.

It is also a means by which caretakers operating on the grassroots level may be identified and moved into service delivery agencies. (An interesting example: one gang-serving agency evolved over time into a pattern whereby the semi-square boys and girls were allocated to a semi-square counselor. The hard-core were allocated to a pinto "street caretaker" type who had been unable to find paid employment for some time because of his long record.)

STREET ETHNOGRAPHY AND THE COLLABORATIVE METHODOLOGY: A COMPARISON

Drawing largely on material in Weppner (1977) and Walker and Lidz (1977), who have performed a valuable service in their unusually candid description of their project, we will develop points of the collaborative model by systematically comparing our processes on five points: setting up shop, the role of the indigenous or community researcher, the role of the professional social scientist, the role of the subject, and the role of the community.

SETTING UP SHOP

For the street ethnographer used by Weppner, setting up shop revolves primarily around the social and ecological establishment of the individual professional ethnographer in the "community" of subjects. (Preble's storefront operation [infra] is somewhat more elaborate.)

To some degree this is what Walker and Lidz (1977) refer to as "validation," and they note the help of the indigenous researchers in helping Walker obtain interviews from normally suspicious subjects. In the Pinto Project, the analogue is the 4 years Moore spent working with convicts and addict groups, but there was no research intent behind that "establishment." Instead, there was a period of mutual exploration and establishment. In the 1974 and 1975 research project, these pinto friendships were mobilized and Moore was "validated" with other pintos, both on the staff, on the street, and in prison. But throughout the lifetime of the research project the research group (both pinto and professional) continued to validate themselves by:

- Visiting the prison self-help groups;
- Inviting visits from both street pintos and prisoners out on "community betterment" trips;
- Distributing drafts of chapters to Chicano prison groups for comment;
- Inviting skeptical pintos from the streets to attend seminars on particular issues;
- Distributing copies of an interim report widely among pintos and community groups;
- Distributing copies of the final report; and
- Writing and making available a book that fully acknowledged the pinto contribution.

Validation is a continuous process because the suspicion of research in the community is profound--and well justified. But it is not only a validation of a particular researcher but of the research activity as a whole in all of East Los Angeles, and not only the pinto "community."

Considering this problem of validation, one must also recognize that the ethnographer validates the subjects to other researchers. This is done partly by publishing and partly by what is told to other researchers. In the case of pintos, they need validation with respect to their willingness to work openly and honestly with researchers; the assurance they are not involved in a ripoff of all square people; and interest in the intellectual as well as policy-relevant understanding of the barrio situation. In addition, pintos are not likely to criticize a researcher without good cause but are likely to tell researchers when they are wrong.

In addition to the validation function, we must recruit both pinto and professional staff before we can set up shop. This is the gatekeeper function. At present, Garcia and some other pintos, who have acquired some understanding of the social organization of professional research and are familiar with our collaborative model, have become quasi-gatekeepers for other pintos interested in a policy-relevant understanding of their own situation. This means that a constructive interest (both staff and volunteer) must be encouraged. But it also means that the impression among some pintos that the project is an opportunity for irrelevant nonsense must be discouraged. It is easy for the lone ethnographer to overlook this gatekeeper function, but in our project the recruitment of other Chicano social scientists is a priority item. Moore has worked at this objective since the beginning of the series of research projects and at present is joined by Salcido and other Chicano social scientists with less formal ties to the project, but who share an interest in action-oriented research with "problem" subpopulations.

Thus, setting up shop in East Los Angeles required extensive recruiting. For the pinto staff we have followed a practice of recruiting ex-offenders from the neighborhoods under study. This maximizes interview reliability. If the interviewer is from the same subculture as the respondent, he can check the reality of the respondent's answers. This happens consistently in the current project and in earlier research. We also have staff members of unusual ability and experience from yet other barrios, recruited by word-of-mouth and personal reputation. We look for people who are respected among ex-offenders--people who have not "burned" other people and have no record as informants or snitches. In general, they are respected by the community as people who can be relied upon.

We look also for trainability--a willingness to first learn techniques and then, to be involved in the analysis. Techniques are learned first; then, as the project moves along, the recruit becomes involved with concepts and with the ultimate product. The staff is encouraged to take courses especially relevant to their work. But, all is not happiness. Several staff were terminated in the past project and several in the present. Usually the cause is poor work, but occasionally it is for involvement in illegal activity. Our staff is to leave the scene if any illegal activity is taking place. Our personnel manual includes sections that provide for action in the case of either the use of narcotics or illegal activity (discouragement for the former; termination for the latter). The project cannot afford to nurture a potential threat to our existence.

Recruiting of professional staff presented problems from 1975 to 1978. However, Salcido (co-principal investigator in the current project) joined us actively in the middle of 1978. He is a faculty member of the School of Social Work at the University of Southern California and responsible for the field placement of social work students in the community. He became strongly interested because of the professional opportunity to develop research-based intervention and treatment models for the most intransigent community

problems and is also greatly attracted by the collaborative model. Salcido has become increasingly comfortable with the pinto staff and with the topics being studied. His interpretive frame of reference has slowly shifted as he has learned some of the values and norms of the pintos. In turn, he has familiarized some of the graduate students in the School of Social Work with the project, thus expanding the network of researchers and credentialled practitioners we can draw on in the future. There are yet other academics who have become interested in the methodology; one has joined the board of directors, one comes to us through an NIMH postdoctoral fellowship; and a third through another postdoctoral fellowship.

Finally, setting up shop also meant (quite literally) mobilizing resources at the community level. Our corporate inexperience meant that we largely depended on community groups. Our office space was donated by an agency sympathetic with our goals which Moore had worked with in the past. Our desks, chairs, typewriters, and other vital equipment were donated or loaned by agency directors who knew Garcia from his work as an active participant in directors' groups. An accountant, in an agency for which Garcia had worked during our unfunded years, volunteered his services. Our board of directors became our inner circle of mentors and advocates; but even more important, the project was surrounded by community resources because they wanted to see an indigenous group "make it" in research.

THE ROLE OF THE INDIGENOUS COMMUNITY RESEARCHER

As Walker and Lidz (1977) state, "an ethnographic study is always a joint production of the ethnographer and his subjects." But street ethnography has several decisions to make on the relationship between the credentialled ethnographer and the indigenous informant-subject. The roles depicted for "subjects" range from those of the standard, unpaid informant (who lays diffuse reciprocity claims on the ethnographer), through the paid informant to the paid employee. Each role presents dilemmas.

- The unpaid informant can legitimately make many demands on the street ethnographer. There are no guidelines on the limits, beyond the energy and the interest of the ethnographer. He often feels exploited by informants and must manage these feelings (Weppner 1977).

  In the collaborative model, many of these problems do not exist. We aim explicitly at a joint production--not "ethnographer" and "subjects" but only the academic-professional and the community-based staff. We pay our staff and we pay respondents. In addition, we expect our staff to help the respondents. They may help with transportation, referral to jobs and services, and other favors. Thus, the sense of exploitation is minimized and staff people respond to normal barrio demands for help.

- The paid informant presents other problems to the street ethnographer: Is he/she lying or distorting in order to get the money (Weppner 1977)?

  Of course lies and distortions occur in the collaborative model, but our strict policy of using homeboys to interview homeboys reduces such problems. Furthermore, there is much overlapping corroboration. We are doing this currently with regard to our history of gang cliques or cohorts.

- The paid staff employee presents a wide range of both opportunities and problems, as discussed by Walker and Lidz. As "facilitator," the indigenous observer may socialize the ethnographers and introduce them to the scene with "credibility," arrange interviews, and steer them. The staff associate employed by Walker and Lidz performed this role well. (There is no counterpoint to the role of facilitator at our project. The project is as much "owned" by the community research associates as it is by the academic associates. Each facilitates the work of the others.)

  For Walker and Lidz, however, the paid staff employee presented many problems. Walker could not persuade his staff that a portrayal of "the world as it appears to the participants" was desirable or possible. Instead, his staff played the "informant" role, presenting subjective accounts as if they were objective and generalizible--a kind of "how to do it" practicality, rather than a complex and fallible internal dialogue.

  In the collaborative model, misunderstanding of the goals of the research can occur at several levels in the staff. At present, we always have staff people working who participated in the early conceptualizations that were funded. New community staff people gradually learn the goals of the research by participating in the analysis. At present, oral history data from our respondents are posted and discussed in full staff meeting; additional questions arise and respondents are found who can answer them. Preliminary drafts are written according to an outline developed by the academic staff in conjunction with the community staff.

  Early in the first project, we found that the "informant" role (presenting personal subjective accounts as generalizable and objective) had to be discarded because our staff consisted of about six pintos--and they disagreed with each other. When one would "play the expert" the others would disagree, perhaps for a special reason, and then the entire community staff would join the discussion. Any tendency toward "expertness" was smothered by the large number of ex-offenders on the staff.

Walker and Lidz found that staff persons were either lacking in writing skills or anxious to use such skills in better jobs. Writing skills are a problem in the collaborative model, but we do not

consider it serious. Material for rough drafts need not be impeccable or even carry the ideas in sequence. Student assistants rarely deliver final drafts, either.

Walker and Lidz found that staff associates as indigenous ethnographers could not deal with methodological issues of validity, reliability, and generalizability. We have had very little trouble with valid information, although it may take a process of mutual education to arrive at the meaning of "validity". At present, we are compiling statistical data on gang cliques going back about 35 years. Each statistic (e.g., the number of persons in the clique, the number using heroin, the number living inside or outside the boundaries of the barrio) has several distinct meanings. As we develop the data, we "uncover" these meanings. There is no doubt that "validity" means uncovering a kind of reality in the minds of the participants. Often we do this serendipitously.

Retrospective accounts are different from onsite observations, and also present problems of selectivity and bias (Walker and Lidz 1977) and (Weppner 1977).

Retrospective data have been the core of our studies in our version of the collaborative model. We find that the more factual the questions, the more willing the respondents, even if the factual demands are rather substantial.

Anecdotes or "stories" are exchanged between community and academic researchers about each others' worlds. Walker notes that such stories are generated by subjects to make sense of their world--and are at the heart of ethnography. Our stories, however, are generated largely at points of misinterpretation on one side or the other and are used for clarification, or to convey the subtleties of a general point.

Walker and Lidz note that when subjectivity masquerades as objectivity, data collection techniques will be haphazard and confined to a narrow clique. It is possible for this cliquism to become factionalism so that the project itself becomes identified with the particular clique that the staff associate represents. If this happens, ethnographers will not be able to establish relationships outside of that clique. It is not "appropriate" for the ethnographer to "correct" the work of a staff associate or to establish a "strong teacher-student relationship" because it would emphasize the authority implicit in the relationship and make the ethnographers more vulnerable to the "hustle" (Walker and Lidz 1977).

We avoid haphazard data collection techniques by structuring both the sample (quota) and the interview guide. Here we depart from the ethnographic approach. Interview guides are important, even beyond the need to produce consistent data. It allows the community staff people to "objectify" their own experience and to distinguish between relevant and irrelevant experiences, both their own and their respondents'. More sophisticated interviewers who are closer to the project goals can pick up clues about relevance

much more easily. But it is important to remember that new interviewers find it difficult to distinguish between the relevant and the irrelevant. Even worse, unless they have a good reason, they find it hard to cut off a friend's line of conversation. As one of our interviewers often says, "OK, but for right now, what we're interested in is. . . ." "OK, but for now, we have to answer these questions they've got down here." Here is yet another reason for questions that emphasize the factual rather than the attitudinal or projective. It is obvious to a respondent why we want to hear about his experiences with job hunting while on heroin. It is far less obvious why they should answer items on a political efficacy scale or play semantic differential games.

We do not worry about cliquishness and factionalism. Our staff members are selected for their reputation and we are reasonably certain they are acceptable. We do not interview well-known police informants or snitches. This is a community project working inside a subcommunity. If we want to know the particular relationship between our staff and a respondent, it is only necessary to ask. We are aware that all participants have specific and discoverable reputations. Academics also have personal and group reputations and affiliations.

We do mutually correct each other and encourage the community staff to get a formal education. We also fire for incompetence.

THE ROLE OF THE PROFESSIONAL SOCIAL SCIENTIST

Weppner's material contains a number of concerns about the role of the ethnographer, expressing an ambivalence that relates in part to the conflict between the participant and observer roles of the ethnographer. Polsky's comment, "don't be one of them," sums up some of these dilemmas. The dilemma involves an ambivalence about the need of the researcher to become identified with the subjects and the psychic, political, and legal risks of such identification.

We find that the collaborative model avoids psychic pressure for overidentification on either side. This may be especially important for some of the Chicano academics working with us. In the academic world, there is a tendency for their colleagues to regard them as universal experts in any or all aspects of life in the Chicano barrios, especially if they happen to be intellectually interested in gangs or narcotics or other problem areas. We found that our Chicano graduate student assistants were relieved to have the burden of being poverty-level, barrio people removed from them. In addition, for Chicano academics from a barrio environment (where physical expressions of machismo are important) a collaborative project with ex-gang men allows a curious and belated overcoming of anxieties about their own acceptance and usefulness.

The collaborative model reduces the legal risk for the professional. The project staff remember very well the consequences of illegal activity. Few of them are interested in illegal activity, and they are not likely to tolerate another staff member placing their freedom and employment in jeopardy. As an extra precaution, we carry liability insurance.

However, there is an additional burden on the professional social scientist not present in most demographic work--the mandates to retain responsible ties to the project group and the community, and to conduct the research and present the findings with these mandates continuously in mind. Political risk is also minimized. This is a community project. The staff accepts their roles as implying an advocacy stance, and although there is some burden of advocacy left to the researcher, he/she is far from alone.

THE ROLE OF THE SUBJECT

The role of the subject in street ethnography is far more complex than can be dealt with in the confines of this paper. Generally this role has been covered in other material. There is, however, one point of difference that should be mentioned. This concerns informed consent--a doctrine that Weppner's conferees suggest will "ruin ethnography." We find that the consent form serves several extremely valuable functions. First, it provides a psychological protection for our respondents, who are "studied" endlessly in the correctional system. Second, it makes a clear distinction between research and police activity. This has an educational function. It may not convince all respondents, but it does point the way toward discussion of the possible uses of research in the community. And finally, it boosts the morale of the convict staff--they can be honest with their respondents. They all feel strongly that respondents should be told what is happening--and why.

In addition, our institutional review board provides us with a chance to make available an array of service resources for our respondents. This board consists of directors of the major health and mental health agencies in the community; they will help if any mental health problems appear as the result of the interviewing.

Finally, we see our subjects as people who grew up with our staff. We cannot look at them only in their deviant roles. We know their mothers and fathers, their wives and children, and the whole array of normalizing family contacts. This is an important difference. We must focus on the whole individual and also the family, rather than on one particular role set. In the case of a community that is badly understood by the larger and dominating Anglo society, this has been enormously productive of new ideas and new insights.

THE ROLE OF THE "COMMUNITY"

Street ethnographers generally tend to focus on a community of deviants (Siegal 1977, citing Leznoff and Westly 1956; Reiss 1961; Becker 1963), although certainly urban ethnographers also study families (Lewis 1965), associational groups and gangs (Whyte 1955; Liebow 1967), neighborhoods (Suttles 1969; Gans 1962) and housing projects (Wilmoth 1960; Rainwater 1970). As Weppner remarks, however, "the street is where most addicts, prostitutes, hustlers and pickpockets are found," and these are the focus of much of the writing to which we are referring here. Thus the community represents a concern for street ethnographers in several respects:

- That the bounds of the community under study be reasonably clear;
- That the data gathered be representative of the community (Weppner 1977);
- That all types of actors in the community, all the different statuses and classes be identified; and
- That "entry into a new community" is performed expeditiously.

The Pinto Project has a different set of concerns with the community. To begin, our meaning of "community" is complex. First, on the macro level, East Los Angeles is a large and sprawling area. When we talk about "community agencies," we are talking about projects that are usually funded by some descendant of the War on Poverty and staffed largely by paraprofessionals, although both the area and the projects have changed drastically in recent years as the number of Chicano health and mental health professionals have increased and begun to attract some significant funding. This has happened to such an extent that they present a meaningful alternative to county-funded mental health and health ventures. The directors of these agencies and many of the staff (both professional and paraprofessional) form a set of networks that go back several decades. This is not a new community. It is old and has long-standing traditions.

Second, on a more local level, "the community" refers to some of the areas within "East Los Angeles" that are bounded barrios with traditional gangs and a relatively clear sense of turf. White Fence and El Hoyo Maravilla display these characteristics and also have local agencies--such as a church, a service center, and a recreational director in a local park. Each barrio has its own internal networks.

Third, there is what the ethnographer might call the "community of deviants," which in our case is difficult to delineate because in the older age groups it includes ex-offenders and people who have been involved in narcotics dealing and other illegal enterprises, perhaps as crime partners. In the younger age groups, there are tightly bounded gangs. Obviously, there are many

networks involved in this so-called "community." But the "community of deviants" is isolated only by stereotype. Its members are generally involved with square people in the barrios, and square people are involved with them on many levels--through family and other ties. This interaction, or dualism, is what we have tried to describe in our book, Homeboys.

As a corporation, we are concerned with research that illuminates ways in which institutional or noncommunity agencies, as well as community agencies and self-help groups, may work with gang youth and adults involved with the criminal justice system. Generally, we hope to move them into fuller integration in the communities. Thus, in "basic research" we find some topics more interesting than others. For example, we were interested in the concept of prison as a total institution. Our previous studies indicate that for Chicano gang men it is not such an institution and that barrio ties are important. Therefore, the three "layers" of the community show enough congruence to encourage programs for systematic intervention. In general, we might say, we are interested in research that will develop rapprochement among the three coexisting "communities" within East Los Angeles.

Thus it is not possible for us to view a "community of deviants" as necessarily or permanently isolated from the other two levels of "community." Our project is structured, from the institutional review board through the board of directors through the staff, to encourage interaction between the levels and between academic and larger policy areas. We are consistently concerned with larger policy--for example, getting social security coverage for prison workers, improving local programming, and individual opportunity in line with self-help principles. Obviously, these are goals and ideals rather than accomplishments and operating norms. But the point remains; we are consistently concerned with the community.

CONCLUSION

Collaborative research is an efficient instrument for gathering data in minority neighborhoods. When such groups are built, they must grow out of the needs and indigenous structures of the community. Once established they must (1) identify themselves with, and speak for the special needs of the community, and (2) return the results of the research to the community as expeditiously as possible.

Most of the operational problems of street ethnography are precluded in the collaborative model represented by the Chicano Pinto Research Project. There are other advantages to the collaborative model, as well as other disadvantages, compared with the emergent patterns of street ethnography; they appear to represent distinctly different approaches.

The Pinto Project collaborative model is not unique and can be duplicated without effort in other communities. Its success, in fact, suggests that it might be considered a normal pattern of research in ethnic communities.

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What an Ethnographic Field Station Looks Like

Edward Preble, M.A.

To ask how you set up and operate an ethnographic field station is like asking how you write a poem, paint a picture, or throw a ball. The answer can be either very simple or very complex. Until now I have taken the simple course by answering: "There is nothing to it"; "Be yourself"; "Use your common sense"; "Just don't be a jerk"--and if the person is one: "Forget it." I was told that such responses would not be adequate for this meeting, so I will attempt to do what I have been avoiding for so long and share with you, to the extent that I am aware of them, some of the attitudes, values, and techniques, and the demeanor I have developed in working on the streets and setting up ethnographic field stations.[1]

Street research methods, which are thought by many to be so difficult for typical middle-class, usually white, educated persons, are fundamentally no more than the norms of behavior adopted by the same persons in other, more familiar settings. For example, upon moving into a new neighborhood in the suburbs, one does not go up and down the street the first day introducing oneself with a hearty: "Howdy, I'm your new neighbor down the street; hope you and the Missus will come over for a cook-out real soon." Nor does one start a new job by inviting one's established fellow workers for drinks or lunch on the first day and start giving them tips on the horses. Instead, he lays back for a period of time being quiet, friendly, watchful, and responsive, and waits for natural opportunities to promote his interests and participation.

The same principles of behavior hold true on the street. In a small book about civilians serving in the British Navy during World War II, the author, S. Corley Putt, offered the following

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advice to new recruits: "Take note of your new surroundings. Perform the routine as you would set about learning any new technique, don't thrust your opinions down people's throats and be even more careful not to stand wide-mouthed to swallow theirs. Underneath your uniform, go on being yourself" (Putt 1943).

Ned Polsky could not have put it any better, and he puts such things very well. In fact, a careful reading or rereading of his chapter, "Research Method, Morality, and Criminology," in Hustlers, Beats, and Others (Polsky 1969), would have made your attendance at this presentation unnecessary, and saved me a lot of trouble. Although I did not learn street ethnography from Polsky, but rather from my own experiences, I would ally myself with him almost completely in his fieldwork theories and techniques. The fact that my ethnographic research is now exclusively in street drug use and criminal behavior warrants, perhaps, separate treatment.

Our current street office, or ethnographic field station, is located on a block in East Harlem. It is a typical New York City slum street consisting of four- and-five-story tenements, an abandoned school, two storefront churches, empty lots, a liquor store, a small grocery store, and three social clubs. About half of the buildings are burned out or abandoned, as are many of the cars along the curb. The liquor store and the churches get the most action, if you do not include the illegal drug markets, which are the stoops, cars, and apartments--depending on the weather.

If one contemplates working in such a community it is important to note one's immediate reaction to the physical setting, even before he meets anyone there. If it is a positive one, even with elements of timidity or apprehension, he is in the right place. On the other hand, if it is a negative one, even with elements of determination and bravado, he does not belong there.

The storefront we rented for our present study had been a small, two-room grocery store owned by members of successive ethnic groups (Jewish, Italian, Puerto Rican), and most recently had been a social club, an institution common in Puerto Rican neighborhoods throughout the city consisting of a nucleus of persons from the same town in Puerto Rico (a club is usually named after the town), but open to other interested and compatible neighbors. The standard equipment and furniture are a pool table, jukebox, makeshift bar, public telephone, toilet, chairs, and a table for dominos and cards. In the traditional club, the members are exclusively men, engaged in social activities--mainly playing dominos, cards, and pool--all usually associated with sociable gambling and drinking.

In recent years, many of these clubs have been taken over by younger first- and second-generation Puerto Ricans with no effective ties to Puerto Rican history or culture. Young men and women both attend these clubs, although attendance is predominately male. The jukebox becomes the center of attraction, accompanied by drinking, smoking marijuana, and, increasingly,

using cocaine, heroin, and other drugs. Some of these clubs have also become drug-marketing centers. With these changes in social club functions, conflicts among the club patrons and with the law have become common. Such conflicts, culminating in a killing, led to the vacancy of the storefront we rented.

The ethnic distribution of the residents in our block is about 50 percent black and 50 percent Puerto Rican; two of us in the research project are the only white, non-Puerto Rican persons living or working in the block. On our first day we could not find the "super" (as resident building superintendents are called in New York) of our building who was to give us keys to the storefront. We asked a lady, who was at an open window just above the store, if she knew where he was. She said she had never heard of him. After looking for him for some time we had to leave. But during our wait on the sidewalk, two small children belonging to the lady upstairs came down and, under the watchful eye of the mother, observed us with shy curiosity. We talked with them, partly in English and partly in Spanish, and left, saying we would see them tomorrow. The next day when we arrived at the store, we saw an electric extension cord hanging out of the window above us down to our front door. The lady upstairs called down and said that when the "super" (whom she now knew very well) let us in we would find no electricity and could use hers until ours was turned on. This offer of electricity and friendship was made simply, without effusion or ingratiation. Her initial guardedness was as quick and instinctive as was her acceptance of us the next day.

The self-preserving observations and responses of tenement residents are quick and keen. Street ethnographers can easily be distinguished from plain clothes policemen, process servers, bill collectors, and welfare investigators; and they are not perceived as a threat. This holds true even when the ethnographer begins to collect personal information of a sensitive, even incriminating, nature. A few months after we had been collecting and recording such information about drug use, drug selling, and criminal activities in the block, Federal, State, and city narcotic agents raided the block and made a number of arrests. No one has ever charged or even intimated that our field station was in any way responsible. On the other side, although law enforcement agents must know about our work and associations in the block, none of them has ever attempted to elicit information from us during our 10 months there. Our experiences in this block relevant to common concerns about acceptance and compromise in ethnographic work are similar to those I have had over the years working in other slum communities in New York, and they should dispel preconceptions that often prevent the undertaking of street ethnographic studies.

Having settled into the physical setting of our ethnographic station--two rooms and a toilet (flushed by a bucket of water drawn from a water tub)--the first task was to identify and establish a relationship with the natural leader of the block. This was not difficult. Every tiger has his hill, as the ethnologists

say, and in a New York City block that hill is often a landing at the top of a stoop; not any stoop, but one particular stoop, usually in the middle of the block. From here he commands a strategic view of the territory.

The unofficial, but effective block leader is almost never a drug addict, professional criminal, or other conspicious person who would attract the attention of outsiders. In our case, it is the super of the building across the street who uses his free time repairing cars in the street. In my first contact with him shortly after we moved into the block, I asked for his help with my car and addressed him as "Mechanico," a nickname which has stayed with him for me and my colleagues. He has since been helpful to us in many ways, although not as a referral source or consultant for our research with drug users. More important than the practical help he has given us with our cars and our store is the way in which he has enhanced our credibility and social position in the block merely by his friendly association.

This is a good time to mention the importance and utilization of nicknames in conducting ethnographic studies in lower class neighborhoods. Nicknames are so common in such settings that not to have one is a mark of social neglect or estrangement. This is true in all the ethnic groups I have worked with (black, Irish, Italian, and Puerto Rican), but with a difference. Among blacks, nicknames are somewhat pedestrian; for example: Eddy (Edward), Hud (Hudson), Tommy (Thomas). Or, with some elaboration: Little Richie, Cigar Slim, Tall Charlie. Among the younger, hip blacks (especially narcotic users), the nicknames are often exotic: Cool Breeze, Cotton Don, Gorilla Johnson, No Red Tape Nate. Irish nicknames frequently allude to some physical characteristics: Beaver (protruding upper teeth), Scramble-eyes (cross-eyes), Tin Ear (hard of hearing), Ratchet Jaw (conspicuous jaw), Bent Over Benny (stooped). Italian nicknames include references to both physical and personal traits: Louis the Beak, Tony No Arms, Mike the Flight, Johnny Dum Dum. Puerto Rican nicknames are often standard colloquial: Pepe (Joseph), Tonito (Anthony)--or references of endearment and respect: Papo, Macho. In one Puerto Rican neighborhood, my nickname was Cinco Peso, short for El Hombre De Los Cinco Pesos, when I was paying five dollars for interviews. Even if it is somewhat derogatory, a nickname is a sign of acceptance and respect, and the person who coins a nickname for someone that is adopted by others is also complimented. The street ethnographer does not have to be the recipient or donor of a nickname to be socially accepted, but he should be alert to the significance and nuances of the custom.

It is at this point that I can expect someone to observe that indigenous researchers are indispensable to street ethnography because for them such customs as nicknaming are part of their social inheritance and can be automatically taken into account in their descriptions and analyses. Although they might have some initial advantage over an outsider in this respect, it is not so important as to justify the often assumed corollary that the outsider is at a necessary and permanent disadvantage. It is this

gratuitous opinion that keeps many aspiring street ethnographers from entering the field.

Not only is it unnecessary to be from the same social, cultural, or racial background in order to do good fieldwork, it can even be an advantage. The very fact of one's difference attracts attention and interest; if, after capturing this attention, one follows through on his own merits, he can do at least as good a job as someone from the same background, and often better. I once took a very proper British Broadcasting Company (BBC) newscaster--dressed in his tailored black overcoat, wearing his bowler, and carrying a tightly wrapped umbrella--to the street corners in Harlem and he got along famously with the young people (gang members and their girl friends) that we met. He had a sincere interest in them and they, in turn, were sociable and gracious without in any way making fun of him. They asked about the Queen and the fog, using their limited knowledge about England to make him feel comfortable. The author of the seamen's text I quoted before tells about another Britisher, an anthropologist, who was conducting a field study in our Deep South:

> Relaxing by no whit his customary standards of conversation he had been accepted by a Negro Family and when without the least fuss or embarassment he had measured the cranium of the master of the house, he gravely handed him the callipers, as from one scientist to another, so that Old Moses could, if he wished, take note of the measurement of an Oxford head.

There are no special personality traits that qualify one for street work. Over the years I have had the opportunity to take colleagues and students into the field for observing and experiencing New York City street life in tenement neighborhoods. Eleanor Carroll, Pat Hughes, and Mark Moore were among them, and I use them as examples because most of you know them. They had very different personalities: Pat Hughes is dynamic and gregarious; Mark Moore is quiet and thoughtful; Eleanor was Eleanor. All three of them made easy and natural contact with everyone they met. What they had in common was a sincere, respectful interest in what they were experiencing, and that is the indispensable requirement, no matter what one's appearance, personality, or cultural background.

To return to the narrative about our entry into the block for the purpose of studying drug users in their natural environment, the next step was to recruit drug users for their research participation. ("Participant," by the way, might be a better term for those whom we work with in these studies than "subject," "respondent," or "informant.") In drug-use field studies, getting participants into the office and signed up is the easiest task one faces--all you need is money. For the reasonable fee of $10.00 for an hour interview, a project can get its quota of participants in a few days, depending on the discrimination of its sample. As one participant put it, "when it comes to money I'm like greased lightning."

Money is usually the sole motive for participants at the beginning, and this is often the cause for professional skepticism about the validity of research findings based upon such a foundation. But if money is a mean motive for those we recruit, it is the same for us who do the recruiting; so we are even. The fact is that one can work with this motive and parlay it into honest, thoughtful participation. If given a chance, everyone likes to talk about himself. (When I made this observation recently--after holding forth on other matters--with a small research group, one person muttered, "So we've noticed.") Sooner or later, if given his head, one will talk freely and sincerely about himself and his activities. If a participant is at times untruthful or deceptive, or so you believe, it is best to let it go unchallenged. He will soon be able to tell by himself whether he is fooling you or not, and whether it is serving his purpose. It is better to run the risk of being fooled than to create an atmosphere of suspicion.

There are some practical measures one can take to relax an interview participant and promote a cooperative relationship. For example, the seating arrangements should be considered. If one is using a table or a desk, it is best to sit an an angle to the person so that one can support a pad or tape recorder and not have the table or desk directly between oneself and the subject. This arrangement makes the interview less formal and authoritarian, and suggests a partnership, which it is. Here one must guard against the other extreme of being so informal that all distinction of roles is lost and the interviewer is perceived as trying to be "one of the boys," which marks him as a "phoney," and rightly so. (Sailors who were "ordinary seamen" used the derogatory term "Equality Jack" to refer to a superior officer who made too strong an effort to be sociable.)

Any strained or unnatural effort to be friendly is suspect and, therefore, unproductive. There are, however, some techniques one can learn without damaging a relationship. One easy and useful habit to acquire is that of using a person's name repeatedly in talking with him. This is a common speech pattern on the street. Thus when a person comes in the door you can greet him with "Joe, how you doing, Joe?" This practice is also important in the interview session. Many of the questions should contain a pause for use of the person's name rather than going from one question to the next as a matter of rote. Thus even three successive questions might go like this:

Q) Where in New York were you born, Joe?

A) .....

Q) Where was that, Joe, on the East Side or the West Side?

A) .. ..

Q) Joe, tell me, was your father living at home when you were born?

A) .....

Everyone develops his own style of interviewing and cannot, or should not, impose it on other interviewers. Any approach that can be recommended should be adapted to one's own personality and manner of discourse. Essentially, it should be sensitive and respectful. Upon reflection and with the help of my interview recordings, I can illustrate my approach by quoting a miscellaneous selection of phrases that I discovered myself repeating in interviews:

We appreciate your helping us out in this work.

Can you give me a little story about that.

Correct me if I'm wrong.

I don't want to put words in your mouth.

Is that a fair way to put it?

If you'd rather not talk about it...

I know these things make you sad, but...

What's the story there?

How would you describe your mother?

Your father, what kind of a fellow is he?

I am not necessarily recommending the use of these phrases; in fact I feel embarrassed at being so didactic. They are only meant to suggest what I consider to be an appropriate tone for the kind of interviewing we undertake in street ethnography. Finally, in interviewing and in all the work on the street, one must have a sense and appreciation of humor. This I can illustrate from an interview without embarrassment because it was conducted by one of my colleagues, Tom Miller, whom some of you know. Miller was completing a first interview with a man who had been convicted of three felonies, including homicide and armed robberies. He had related details about the commission of these and other crimes earlier in the interview; this was the concluding exchange:

Q) What have you been doing since you got out of jail this time (one month earlier)?

A) After I got out this time I haven't done nothing. But I've been working; you know, checking up a place. I'll tell you the truth. So if I get ready I am going to go in there and after I go there I am going to tell you so then you can read it in the papers. You know what I mean? You'll know it's me.

Q) Yeah.

A) When I tell you to read it in the paper, you can report it to the police if you want to but they won't have nothing but your word and my word. So right there they got no case. When I go in the place they ain't going to see my face, they ain't going to see nothing.

Q) You don't have to worry about my telling anyone, what we're doing here is strictly confidential and your rights to confidence are protected by law.

A) Yeah, and I could sue you for that.

Q) Sue! Sue would be the farthest thing from my mind. You might get a gun and shoot me.

As I look now, at this moment, at the pictures of our block in East Harlem the fragment of a poem by T. S. Eliot comes to mind:

> The conscience of a blackend street
> Impatient to assume the world

These lines suggest something of the mysteries and the challenge of our work.

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A Four-City Study of PCP Users

Methodology and Findings[1]

Michael Agar, Ph.D., and Harvey Feldman, Ph.D.

Michael Agar: When I talk about ethnographic research, either in its proposal form or as a complete project, folks usually ask the standard questions: What are the variables? Where are the hypotheses? Where are the operational definitions? Where's the instrument? Where's the sample design? As one of the Birch & Davis' employees so succinctly and accurately put it when those questions were posed, "You don't understand. These folks grow their own." Sometimes I think ethnography is to the social sciences what jazz is to music. Well, I like jazz.

Regarding the outbreak of PCP use, what occurred, as it so often does, is that a lot of folks got nervous about something that was obviously going on and that people were talking a great deal about. In response to the PCP scare and the talk, they developed a very formidable response indeed. The problem was that the things they were talking about and doing were based on assumptions about what the PCP users "out there" were like. The fact is they really didn't have very good information about what they were like. So, six ethnographers worked with the staff of the Services Research Branch of NIDA to organize and implement an ethnographic project designed to study PCP users in their everyday life, a study that asked simply: What are they doing out there? What are they like? What do they care about? What do they talk about?

We faced the immediate problem of not having much money and not much time. This is a problem ethnographers will have to contend with more and more as they begin to deal with policymakers who

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[1]The complete study is being published under the title of Angel Dust: An Ethnographic Study of PCP Users (Lexington, Mass.: Lexington Books, 1979).

need information about certain populations and cannot wait the 2 or 3 years ethnographers would normally like to have for their studies. What we tried to do in a 3-month period was develop an exploratory study of PCP users.

First of all, we decided to go with professional ethnographers who had established a research base already and were in contact with either PCP users in particular, or with drug users in general. We were fortunate that four experienced and highly qualified ethnographers were available in four different cities--Jim Walters in Philadelphia, Wayne Wiebel in Chicago, Patricia Cleckner in Miami, and Jennifer James in the Seattle/Tacoma area. This permitted us to have the kind of geographical distribution that allows us to call this "the first cross-national ethnographic drug study." In contracting with four ethnographers with established contacts in their cities, we were able to cut off the usual 3 to 6 months it usually takes for an experienced ethnographer to gain entree to street groups. Keep that in mind when we discuss the results. You usually cannot do this type of study from scratch in a 3-month period unless you have these kinds of qualified and experienced people available.

A second problem was the time constraint on the study itself and the limits this placed on the range of information the ethnographers could collect. A few of the ethnographers who did the study felt frustrated. Although they learned a good deal about users and the way they lived, there were many other things they didn't have time to address.

The third problem, which is the classical one ethnographers have traditionally faced, is to distinguish between description and comparison. One of the strengths of ethnographic work is the flexibility to adapt to the particular circumstances of the moment. You eventually learn how to ask the right question in the right way. You also have another goal, which is to communicate the results in a manner such that comparisons can be inferred with other people who are doing studies of similar groups in other parts of the world. What we had was four ethnographers in four cities who had to talk to each other about the results. Therefore, we wanted specific features of the groups in Chicago, Philadelphia, Miami, and Seattle; and at the same time, we wanted to be able to make some general and comparative statements at the end. To try to accomplish that, we developed a manual and several data collection forms. Again, we cheated because we had people who not only had extensive experience carrying out ethnographic studies, but people who had studied PCP users. In essence, we had a kind of pilot study already underway. Through the manual and the data sheets we were able to develop a strategy for collecting comparable data in all four cities.

It should be understood that this study had some special conditions that placed constraints on us, as well as some resources that are generally not available (experienced researchers already in the field), which allowed us to get a head start.

Harvey Feldman: The PCP study does raise many questions about the present perspectives on the dangers of PCP and the way this drug has been presented by some treatment personnel and the mass media. We are all aware of the range of horror stories that have been attributed to PCP. It has been viewed as the new monster drug bursting on the scene in 1977 as a drug of choice for many young people. In the PCP study, four ethnographers met and observed more than 300 PCP users and interviewed 100 of them in four cities. We found that much of the history of the drug's use is somewhat inaccurate. In addition, much of the way users have been portrayed is misleading, at least if the drug is seen from the users' perspectives. With that in mind, I would like to provide you with some highlights of our four-city ethnographic study of PCP users.

In the study of PCP users, we were able to make some observations about the way a new drug enters the youth culture and spreads through it, although much of this data was retrospective rather than observational. There are gaps that perhaps future research may fill. For now, we are able to note that PCP is not a fad drug--meaning that it appeared, became popular for a period of time, and then vanished. As Richard Blum noted, new drugs, once they come into use, almost never go into extinction (Blum 1971). The street history of PCP illustrated this observation and may well serve as a theoretical model to show how a new drug is introduced, enjoys a period of enthusiastic popularity, and then loses its glitter and becomes incorporated into the street pharmacopeia of drugs and chemicals that users continue to consume. With PCP, the pattern had three historical phases: the introduction, the spread, and a period of stabilization.

THE INTRODUCTION

Most of the popular reports on PCP have indicated that its street use began around 1967 in San Francisco and a bit later--around 1968--in New York. The drug, so the reports go, quickly developed a bad street reputation and did not become popular. For a lengthy period of time, approximately 1968 to 1975, PCP was sold as a drug of deception and was passed off as LSD, cocaine, or THC, the active ingredient of marijuana.

Our study indicates that PCP emerged in our four cities at approximately the same time it appeared in New York and San Francisco. In the late 1960s, the drug was taken in pill form. In Philadelphia, the pills were called "t-tabs"; and in Miami and Chicago, they were sold as THC--a drug that was never available in the illicit marketplace. Whether users ever believed that PCP was actually THC is not known. What is known is that the powerful anesthetic effects were associated with THC and began to pick up street names that indicated this association. In Chicago and Miami, PCP was called "tic," a kind of bastardization of THC and an acronym that could be pronounced. In Philadelphia, it was called "titch." From our reports, it seems apparent that few young people

understood or even cared about the deception. Our data strongly suggest that the deception was not an important factor and that young people bought and used PCP quite willingly, even though the name of the drug indicated some confusion on its actual derivation.

In pill form, however, the drug proved to be more powerful than users could manage, although in Philadelphia the pill form was considered too mild. Eventually young people learned how powdered and crystal forms could be better controlled through smoking and snorting, especially snorting.

This simultaneous appearance of PCP in different parts of the country is one of the surprising findings of this study. Although it was not possible to explore how this phenomenon took place, we were able to note that two components were necessary before information about a new drug could move so rapidly across the country without the aid of the mass media: a communication network that is presently not well understood, and literally thousands of existing social groups with members who are willing to experiment with new substances that promise a new, different, or unusual experience. Although we were aware that this orientation existed among young adults and older adolescents, we were surprised to note the same, and perhaps even greater, enthusiasm among younger adolescents in the elementary and junior high school level.

THE SPREAD

The second phase of the street development of PCP use took place approximately from 1973-75 when use of the drug spread throughout each of the study cities. Once the drug was introduced, local patterns tended to alter and modify the ways it was used. Users soon learned that the effects of PCP were too extreme. The heavy anesthetic effects that put users in an extended comatose state were considered undesirable, although other psychedelic aspects of the experience made it enjoyable. The potency of the drug, however, was viewed as a challenge and managing it became part of the lure that made an otherwise unpleasant experience the kind of thrill that helped young adventure-seekers establish local reputations among peers. In other words, young users liked many aspects of the experience, but preferred to enjoy them in a more subdued manner. Through experimentation, they learned that different routes of ingestion could alter the nature of the high. Smoking produced a lesser effect than pill form; and snorting made the experience even more controllable. It was during this phase, after street strategies had been developed to control the effects of PCP, that enthusiasm for PCP was at its height.

During this phase of spread, when enthusiasm for PCP was rampant among the street participants in Chicago, high status was accorded those youth who were daring enough to chance the odd,

anesthetic effects of PCP. Taking a tranquilizer that was intended not simply for animals, but for large animals such as lions, horses, or elephants, and managing the aftereffect brought status to those young people involved in a peer system where risk-taking was honored and respected. During this early period of spread, being regarded as a burn-out was considered a form of status enhancement.

THE STABILIZATION PERIOD

Even though PCP was known to have a powerful impact, the long-term effects did not show themselves immediately. In all four cities, however, concern about the symptoms of burning-out eventually developed. It was the concern about burning-out that was the most important inducement to develop street strategies to manage PCP. The belief that extensive use of PCP could cause permanent damage, particularly brain damage, led to its decline as a favored street drug. Like a new craze of any kind, PCP eventually lost its fascination. Rather than fading completely, however, it remained available, because moderate use still provided the kind of fun and adventure many young people desired.

Today, most drug users take a realistic view of PCP, recognize its power, and make the necessary adjustments to manage it. The more common pattern of use would be to consume a small amount, sufficient to provide the kind of psychedelic experience users can enjoy in a conscious state. Users tend to limit use so that burn-out symptoms do not appear. If they do appear, users are known to cut down on the amount they use or to discontinue use altogether. Although PCP is still one of the most frequently used drugs among young people--second to alcohol and marijuana--and has been accepted as part of the arsenal of available intoxicants, it is no longer a highly preferred drug. Its frequent use would be attributed more to its availability than the attractiveness of the high it produces. During this period of stabilization, there were, of course, new users introduced to PCP. But apparently there has been a decline in the number of new users, as well as a decline in the level of enthusiasm the earlier experimenters brought to the activity. This period of stabilization has run from approximately 1976 to the present.

THE READY MARKET OF USERS

The spread of any new drug requires not only a substance with some appeal and the promise of an exciting high, but also a reservoir of users who are willing to sample the drug and experience its effects. Who, one may ask, are these willing experimenters? How do they manage to challenge a drug like PCP when the true and apocryphal stories about its potency are frighteningly accurate and known? Previous ethnographic studies of drug users provide some hints to the willing acceptance of a whole

variety of adventurous activities. This eagerness for thrills was a feature that Preble and Casey recognized underpinned the street action of New York heroin addicts (Preble and Casey 1969). The title of Michael Agar's ethnographic study of heroin addicts at the U. S. Public Health Hospital at Lexington--Ripping and Running--reveals the tone and tenor of the lifestyle (Agar 1973). Sutter clearly described how young black dudes, as they called themselves, in Oakland, California, strove to be "the baddest" in a world of action and violence (Sutter 1966). These same features of restlessness--a search for something to break up the monotony, a kind of edginess that allows an eager acceptance of something new and different--exists now as an undercurrent in the middle-class youth culture. Although violence and other assaultive behavior have not been completely incorporated in the middle-class way of life, the thrust into drug use has brought middle-class youth closer to the world of the criminal and the underclass than any other adventure.

In all four cities, the dominant group that explored and pioneered PCP was the white adolescent, both working-class and middle-class males and females. Among the white groups we studied, there existed a kind of restlessness, a search for action, and a sense that life generally was boring, uninteresting, and lacking recreational alternatives. Walters noted, in the Philadelphia area he studied, that "suburbia lacks spontaneity." But whether the setting was suburban Philadelphia, where cars and telephones connected the youth system, or inner-city Chicago where youth groups historically congregated on corners and milled around mom and pop sandwich shops, the theme of looking for action permeated. One of the core pastimes of these groups of young people has been not simply taking drugs, but the complete range of activities that surround and are associated with it--a range of activities that always breaks up boredom and brings a faster pace to life. Perhaps the most enduring legacy of the youthful social upheaval of the 1960s has been the commitment to drug experimentation as a part of growing into adolescence. The drug use the hippies and other members of counterculture groups that began in selected urban areas such as New York's Greenwich Village or San Francisco's Haight-Ashbury has now become indigenous to almost every community in America. In fact, today's youth are the inheritors of the previous generation's knowledge of drugs and their effects, so that many of the mistakes past generations made have been corrected and many of the dangers knowledgeably avoided. In other words, the present generation of drug users, despite the innocence usually associated with youth, accepted the heritage and have in their collective possession highly sophisticated information on the selection, distribution, and management of several dozen different substances, of which PCP is merely one of the most recent.

We have had difficulty drawing up a profile of the PCP user because our study indicates that they are rare in pure form. Closer to reality is the adolescent, male or female, who identifies with a group of peers who collectively look favorably on the use of drugs as an integral means of enhancing life. These young

people place a positive value on illicit drug use, understand the dangers, and are willing to risk them. In fact, the risks themselves may be one of the more important features that motivate use in the first place. In this sense, there are really not PCP-using groups. Instead, they are groups who use a broad spectrum of drugs. Because PCP is readily available and provides the essentials for an acceptable drug adventure, it has become one of the many drug options from which young people can now choose. Because it is easily manufactured, relatively inexpensive, and presents a challenge of the first order, it ranks high among the drugs the youth we studied use, trailing only marijuana and beer in frequency of use. The dangers it presents, however, particularly burning-out, keeps it low on the list of preferred drugs. Most young people use it either sparingly on a regular basis, or use it for binges with other drugs on weekend nights when they seek high adventure and action.

The groups that have included PCP use on a regular basis did not add drug use casually to their recreational options. Rather, they took to the process of getting high--planning the events, financing them, purchasing the drugs, and using them--with excitement, no matter how earnestly their cool lifestyle might attempt to conceal it.

In our study, we noted that socioeconomic status was a key variable in determining not simply the selection of drugs, but also the manner in which the drug was experienced. No matter what their socioeconomic status, however, all the groups had developed an identity that was largely associated with drug taking. In Philadelphia, it mattered little whether the groups were called "cools" or "rowdies," except that the different lifestyles tended to affect the intensity of PCP involvement. Each of the groups saw themselves as heavily involved in drug use that was controllable within the groups' prescribed limits. No groups placed any invidious meaning whatever on the reputations they had developed as drug aficianodos. In Chicago, the group referred to themselves as "freaks," not in the old sense of being oddities, but in the 1960s meaning of having a dedication to taking drugs and enjoying both the high and the reputation. In Miami, Cleckner called her upper-class group "Cognoscenti"--meaning having superior knowledge. Members of that group referred to themselves as "trip stars" and had in fact explored a wide range of highs in search of the best that drugs, narcotics, and chemicals could offer. Having the necessary financial resources, they attempted to construct a drug experience in keeping with their pursuit of excellence in other aspects of their lives. In Seattle, in addition to the usual adolescent groups found in the other cities, there existed a kind of rag-tag assortment of young adults who seemed to live on the fringes of the working-class society. They were people who had found a common identity in helping each other through the various trials and tribulations of broken marriages, employment crises, arrests, court appearances, and drug misadventures. In all the groups, young adolescent or young adult, drug use played a key role in holding the group together, not only in the sharing that accompanies the process of purchase,

use, and enjoyment of the high, but also in the fellowship that arises out of the shared experiences that are sometimes frightening but impart a sense of solidarity when mutual help keeps members from completely running aground.

The usual view of drug users, especially young drug users, is that they are victims. They are usually portrayed as morally weak, subject to peer pressure, and so hungry for group affection that they blunder unknowingly into drug experimentation that stupidly puts their health and lives in danger. The public response has been first to protect them from predators--whether those happen to be adult drug sellers or close friends.

Whether or not this has ever been an accurate portrait of young drug users we cannot say. Our study, however, showed that it does not apply easily to the members of the drug-using groups we studied in four cities. Instead, the young users we observed and interviewed possessed a rather sophisticated knowledge of various drugs, even if they could not demonstrate that knowledge in a language that was always scientifically accurate. Their enthusiasm for new drug experiences was not manifested as blind pursuits that took them recklessly into danger, although danger was clearly one of the aspects of drug use they found appealing. Even more appealing, however, was meeting the challenge of the risk and skillfully overcoming it. The more successful drug users--those who learned how to manage PCP--did not require the services of treatment programs. As a result, they seldom appeared as case material or as statistics. And because almost all of this knowledge was developed before the widespread use of PCP was known by the authorities, it can be assumed that the sophistication about PCP, if not about other drugs, was acquired on the street.

THE STREET PERCEPTION OF PCP

Previous literature on PCP had indicated that its effects are unpredictable. In fact, the word "unpredictable" tends to be one of the words most often used to describe phencyclidine. It refers both to the behavior of users and to the quality of the drug sold in the illicit market. Similarly, the drug is reputed to be sold under literally dozens of different names, which--according to reports--has made identifying it for research purposes difficult because users frequently know it only by its street name rather than its generic name. Although it was true that users often did not know that drugs carrying different names were PCP, each local area had developed a relatively stable way of identifying the various forms in which PCP appeared on the street, although new forms were constantly being introduced. The assortment of names was not totally random, and the state of identifying PCP was not completely one of chaos.

In each of the cities, there have been attempts to attach a street name to the various forms of PCP, in much the same way that

different names were given to LSD in different strengths and forms. This attempt at uniformity allowed the user at least some rudimentary measure of the quality and potency of the drugs he purchased. In Seattle, for example, the name "crystal" was given to the purest form of PCP sold in that city. Its name matched its appearance because it was white and crystalline. Crystal flakes, a slightly weaker form, was similar in appearance but had a kind of oily texture. Angel dust, or dust, was considered to be a good quality PCP that was cut with corn sugar, while rocket fuel was yellowish, moist, coarse, and considered low grade because it was the "dregs"--the left-overs of a better grade. Monkey dust was a brown powder that contained lactose. Because almost all the PCP available in the Seattle area was manufactured by local amateurs, chemists reported the quality was unknown and as a result unpredictable until the batch had been tested. But even though mistakes were possible, the names given locally would cover the range of available forms of PCP. In Philadelphia, on the other hand, PCP was available in two forms: leaf and powder, or crystalline. All PCP in leaf form was called "angel dust" if it appeared on parsley or mint leaves; it was called "killer weed" if it appeared on marijuana. In the powdered or crystalline form, it was called "buzz," "PCP," or "THC," but generally was referred to as "buzz," undoubtedly because of the onomatopoetic association of sound it created in the user's head. The crystalline form of PCP could be manufactured in a variety of colors and textures. In Philadelphia, there was brown, yellow, and white PCP. In Chicago, where it was called "tic," it was also available in a variety of colors, with a special green PCP put out for St. Patrick's Day.

THE SUBJECTIVE EFFECTS OF PCP

Probably no drugs, not even heroin, which for 50 years was heralded as the ultimate monster drug, has been considered more dangerous by drug abuse experts than PCP. It has been presented to the mass media as though it were a live enemy, capable of inducing the young people who experiment with it to lose complete control of their rational faculties, to be so overpowered by the drug that they helplessly and inescapably move directly to either a psychotic episode, suicide, murder, or a state of suspended confusion that only an indefinite commitment in a mental hospital will reverse. Even in its less potent forms, the drug has been reported to cause users to become belligerent and assaultive. In fact, some experts have cautioned emergency room personnel about treating PCP users without the help and assistance of police because of their inclination toward violence. One nationally known figure stated that the effects of PCP are so profound that when he was asked to consult with the United States Air Force, he suggested that any soldier found to have used PCP even once should be discharged because of the possible brain damage the drug can cause.

Perhaps the most important aspect of PCP that helps explain the subjective effect on users is that it is an anesthetic whose effects are dose-related. Cleckner noted among her users in the Miami area that the user's set toward the drug experience was an important influence in the way the drug was experienced. In other words, what the users expected from the drug conditioned the way the effects were eventually perceived. When the drug was expected to provide an enjoyable experience, the emotional response to that experience, no matter how repugnant the content of it might be to others, was interpreted in a positive light. Later, we shall describe how even thoughts of death, which was one of the common themes of a PCP-induced state, could be enjoyed and defined as pleasant.

Similarly, the social context in which the drug effects were experienced was a key factor in shaping the subjective response. Most users preferred a context that was free of confusion, unexpected disruptions, and other people who did not understand how becoming spacey, self-involved, and disoriented could be fun. Under these ideal circumstances, PCP was not an unpredictable drug. One had only to arrange the setting appropriately, look forward to enjoying the bizarre effects with a positive attitude, and make certain that the potency of the drug was properly assessed.

As stated earlier, the effects of PCP are dose-related. Users who have experience with PCP learned that the different dose levels produced qualitatively different conditions. Walters has classified these levels into four categories: buzzed; wasted; ozoned; and overdosed. In the buzzed state, the user feels a mild euphoria, and rather than experiencing the drug as an anesthetic, she/he feels a stimulation so that physical activity is often pleasurable. In this light state, users were known to attend school, take and pass tests, work at physical labor, and otherwise maintain a front of behavior that appeared straight and drug-free.

When a user was in a condition of being wasted, the drug usually caused a body-wide anesthetic effect in which the users felt the effects profoundly in their legs and feet. Typically, she/he found coordinating body movements difficult and speech somewhat slurred. Users described the sensation of walking as particularly amusing because it seemed like the ground had turned to sponge or marshmallows. In this state, users recognized that body movements were slowed, awkward, and unbalanced. In contrast to the outward awkwardness, users sensed a speeding up of thought processes and seemed to enjoy an odd sensation of being able to participate and observe themselves in what has been described as an out-of-body experience.

The ozoned state is one in which the user becomes incoherent and immobile, although still conscious. In the overdosed state, the user loses consciousness, a condition that most experienced PCP users did not believe was life-threatening.

Arranging the setting with all the features of proper use in correct order, however, was not always possible. The amateur

did not know how to carry off such a plan, although it was still surprising how few respondents in the four cities reported having experienced ill effects during their first use of PCP. The younger and less wealthy user had few ways of assessing the quality of a batch of PCP prior to using it. The older group of "trip stars" in Miami were sufficiently educated and wealthy to know how and where to purchase quality PCP, and then to purchase it in sufficient quantity so that they store a supply whose quality and potency were known to them. The groups with fewer resources usually either tested the drug at the point of purchase or requested from the dealer some accurate appraisal of its potency. Both methods were less than foolproof.

One of the aspects of PCP that has perplexed some of the experts has been the way in which users claim to have different reactions to essentially the same drug. It has been reported often that many users associate the effects of PCP with a psychedelic experience similar to LSD, while others enjoy it for its barbiturate-like depressant effects. And to confuse the matter even more, other users insist that their experiences are unique and cannot be compared to any other intoxicating substance.

In addition to the effects that are dose-related, it was found that groups with different social styles sought different behavioral outcomes. For example, in Philadelphia, Walters noted that the cools in his study, those who enjoyed a kind of controlled aloofness in other aspects of their lives, tended also to maintain control over the effects of PCP, using it often to help them carry out activities of work, school, and athletics. The rowdies, on the other hand, preferred the depressant effect. Instead of an evening of psychedelic fun, the rowdies preferred the ozoned state where the loss of coordination coupled with an agressive attitude sometimes caused them to exhibit the kind of bravado that involved them in manly fights in which they were the inevitable losers.

For those who enjoyed the psychedelic effects, the major problem was to adjust the dose to match the expectation--to achieve either the buzzed state or the wasted state without slipping into the ozone. When the proper state had been achieved, the experience, no matter how strange it may seem to persons who have never attempted it, almost always was defined in positive terms. The difficulties in speech, the inability to coordinate movement, or to organize sequential thought made the user appear to be incapacitated and incapable. But although the outside movement may slow, the internal awareness took on a heightened sensitivity and alertness to the condition itself, so that the total experience--rather than being frightening--was usually interpreted as something amusing, like looking at the world as though it were reflected in the distortions of fun-house mirrors.

ADVERSE REACTIONS

No matter how sophisticated young users have become, they all recognized that PCP was a powerful drug. In almost all of the popular and professional discussion of PCP, its potency and the dangers are so heavily emphasized that it becomes difficult to understand how anyone could risk taking a drug that offered such extreme dangers and so few pleasures. The dangers that PCP users recognized as part of the risk of using this powerful anesthetic were not the same dangers that experts in the drug field warn against. For the experts, the issues of social control appear to rank high as concerns; much has been written about the aggressiveness and violence associated with PCP use and the need to contain it. From the users' perspective, these events were too infrequent to be worthy of notice. Users simply did not associate violence with PCP. They did, however, have other concerns. These fall into three general categories: taking too much; disrupting normal routines; and burning-out.

The possibility of taking too much PCP in any given attempt was always a threat. Except for those who actively sought a heavily anesthetized condition, the aim of most PCP users was to control the effects so that the ozone state was avoided. Although some users developed methods for moderating the high, the tactics were not foolproof unless users had the necessary resources to maintain control over the quality and potency.

Except for groups like the Cognoscenti in Miami, users were generally unable to buy in the kind of bulk that would assure quality control. Instead, the typical user would purchase enough to last for a day at a time, or only for the afternoon or evening. With such variability in the supply of PCP, the strength fluctuated. Unskilled users would not know until they had used the drug just how potent any given dose of PCP actually was. Because the effects of the drug are dose-related, it is not surprising that users, particularly those in the years when PCP was spreading, had qualitatively different experiences each time they experimented with it. Even though the drug developed a reputation for being unpredictable, the effects were actually quite predictable, provided the strength and amount of PCP were known. Users, however, especially young ones, seldom had the necessary resources to control quality and potency.

One of the consequences of taking too much was the bad experience that was always unique to the individual user. Some users simply did not like the kind of mental and body distortions caused by the anesthetic reaction. Even though users often compared the PCP experience with LSD, there seemed to be a qualitative difference. With LSD, the existing reality was often distorted in a swirl of colors and images taken from the surrounding world. Users drew the distinction between changing the reality with LSD and creating a totally new reality with PCP, a condition they called hallucinating. The content of the hallucinations was often connected to changing body images--shrinking legs was a

common theme--and a feelng of being isolated and almost invisible. When these experiences became distasteful, users were often frightened, sometimes cried, and frequently became depressed. Often these negative experiences could be altered simply by flowing with the sensation of them until a different perspective on the same condition could be developed. When this was done successfully, rather than considering the distortion a handicap, the user could switch emotions, leave fearful feelings behind, and begin enjoying the same experience that had only moments before caused consternation.

One of the more consistent hallucinations that PCP apparently triggered--and this same phenomenon occurred in even the early studies when PCP was being experimented with as a legitimate anesthetic--was a sense of death, called in the scientific literature "meditatio mortis." Users often reported this as content of their intoxicated state. Although their reactions were often fearful and triggered periods of crying, they often found the experience exciting and enticing. It was reported as a pleasant kind of out-of-body experience that made them feel like the "living dead."

These and other adverse reactions to the drug could be quickly reversed and converted into a positive experience. Frequently users could not decide whether a frightening experience was negative or pleasurable. In Chicago, for example, Wiebel reports that users often stated the contradictory view that the best experience was the worst experience. Fun and horror were blended, so it appeared that adverse effects were not simply a hazard associated with use of PCP but in fact one of the pleasures sought from it. By having frightening tales to tell, users could demonstrate their own daring in using a highly potent drug. They could meet and face an internal scene of terror, then return from the vision and bring back to their friends a war story rich in hallucinatory detail. In this context, the worst could be the best even if, especially if, the experience seemed to take the user close and sometimes directly into death itself.

If the experience was too unsettling and threatened to become a "bummer," friends were always available to calm a user and provide quiet reassurances that distortion would disappear as the effects of the drug wore off. Depending on the geographic area of the country, friends would offer either milk or orange juice, try to make the user vomit, or suggest breathing exercises as methods for managing adverse reactions. Among the most sophisticated users supplies of Valium provided a chemical counteraction. But usually just allowing the person to wait out the drug's effects in a quiet, nonthreatening environment, one as free of stimuli as possible, was sufficient to bring the person around.

Of all the adverse effects that users reported, the one that concerned PCP users the most was "burning-out." It was abundantly clear to all the users that sustained and regular use of PCP would lead inevitably to burning-out, a condition that was described as appearing "spacey." In this condition a user was

actually incoherent, unable to think clearly, and experienced severe memory loss. As a result of these symptoms the user generally developed a reputation for being unreliable and lacking in fun and spontaneity--characteristics that run counter to those features adolescents tend to value.

In the early days of the spread, Chicago users, caught up in the enthusiasm of exploring a new and powerful drug, placed high status on being a "burn-out." As the condition became better understood on the streets and users began to recognize the handicaps of memory loss and an inability to think clearly, the status dropped steadily. In today's world of drug users, the PCP burn-out is viewed with a mixture of pity and disgust. In all four of our study cities, the burn-out was ostracized and demeaned. Groups of users had developed a system of values that was designed to protect members from becoming captured by drug use. Inherent in this ideological scheme was the belief that drugs and chemicals should provide a route to fun, excitement, and danger; but they should not be allowed to destroy the physical, emotional, and mental capacities of users. In understanding this aspect of the young's approach to drug use, it can then be seen that they are receptive to educational information about drugs, provided the information is not intended to dissuade them from drug use altogether. In their own way, this was the strategy they themselves had developed in managing burning-out and burn-outs.

In order to reduce the likelihood of burning-out, users generally made a conscientious effort to keep use of PCP under control. They watched themselves and each other for signs of burning-out. When these signs appeared--and they were always clearly visible--users frequently began either to cut back on the amount of PCP they used or to quit it altogether until the symptoms receded. For users who were unable to cut down on their own, other group members generally pointed out the symptoms in an attempt to alert a user that she/he was in danger of becoming a burn-out. These attempts to alert the prospective burn-out came in the form of warnings. When the warnings failed, humor and ridicule were applied. If the user still did not respond to these group sanctions, she/he was eventually labeled a burn-out and was either scapegoated and/or ostracized.

THE CRIMINAL JUSTICE SYSTEM AND TREATMENT

Despite almost daily use of drugs and frequent public displays of noisy behavior, there were few arrests of the PCP users studied in the four cities. As a result, few of the participants in our study became involved with the criminal justice system or developed any familiarity with the court or corrections system. Because of this, these polydrug users generally did not think of themselves as criminals. The few exceptions to this generality were the burn-outs or those with a commitment to rowdy behavior. It was not the drug use but rather the offender's reputation as a troublemaker that caught police attention.

There were clear indications that the police in each of the cities gave low priority to harrassing or arresting the street-level drug user. Just how much this policy has become an official one within city police departments we cannot, of course, state. Their apparent acceptance of drug use among young people, however, was consistent with reports about the way school authorities and employers responded. The everyday practice would tend to suggest that drug-using behavior has become an accepted, if unwelcome, fact of life. In Chicago, for example, rather than arresting young drug users on charges of illegal possession, police simply confiscated the drugs and warned users against future violations of the law.

The only two exceptions to the practice of overlooking the behavior of drug users were in those situations where drug users became involved in behavior that disrupted public order or were caught in what appeared to be life-threatening situations, such as overdoses. If police were called, they had no choice but to respond to complaints. This practice seemed more typical in the interaction of police and rowdy groups than with other groups less prone to public displays of disruptive behavior. Even in those circumstances, police tended to manage them like they managed family quarrels and acted as mediators rather than responding as though serious criminal activities were in progress.

When police were involved in overdose situations, they turned the matter over to medical professionals, usually those in the local hospital emergency rooms. In those few instances when users suffering from an overdose of PCP were taken to hospitals, the condition of the patient was reported to have worsened with medical intervention. Within the drug users' network, the usual practice was to allow the user to wait out the effects of the drug and to keep him and the environment as calm as possible. It appeared that emergency room management of PCP users sometimes exacerbated the situation. In most situations hospital personnel had been informed that the typical PCP user was prone to violence and assaultive behavior. As a result, users were frequently put in constraints. In all cases where this happened, the treatment experience became a nightmare for the user.

Most members of each of the drug-using groups viewed treatment programs as extensions of the criminal justice system. Treatment programs, for the most part, were regarded with distrust and suspicion and used only as a last resort. Emergency rooms, for example, were thought to be staffed by incompetents who probably would make an adverse reaction to PCP worse.

Almost all of the users we studied did not think they needed to change because few of them believed that their use of drugs had caused chronic problems they themselves could not handle. As a result, there was little interest or incentive to seek the kind of help or counseling offered in drug treatment programs. Because few of the respondents had been arrested, there was only limited data on involuntary treatment. Because the data were so sparse, little can be said about experience with treatment programs other

than that it was the general assessment of the four ethnographers that in the few cases they observed the outcomes were not particularly successful.

VIOLENCE AND PCP

Because the mass media has associated violence with PCP, this topic was one the four ethnographers were requested to explore in as much detail as possible. Each of them was asked to report on any first-hand observation they made during their 3 months of data collection. In interviewing respondents, they were asked to inquire about violent episodes the interviewee had either participated in or had witnessed directly. These episodes were to be explored in detail. Finally, users were asked to report any hearsay stories of violence that were reported to be triggered by PCP.

With such a concentrated focus on the issue of violence, it seemed natural to expect that our study would generate considerable data on the association of PCP with violent acts. In almost all cases, however, PCP users were baffled by the connection of PCP and violence. Most of them believed that PCP was so powerful that the kind of coordination and agility required in a fight would be lost. In fact, most of the illustrations of users becoming involved in what they perceived as violence were usually humorous stories in which users were the injured parties.

Violence was not completely absent, however, although it appeared to be rare. It tended to appear among those groups where toughness was important in developing and maintaining group status. With groups like the Rowdies in Philadelphia and the corner group in Chicago, fighting was a traditional assertion of status--a means not simply of resolving personal conflicts, but of presenting the kind of tough image that was respected locally. In this sense, violence that an outside observer might attribute to PCP was actually behavior common to the group and indigenous to the community long before PCP had become a favored drug. Given different behavioral orientations, then, groups varied in the emphasis they gave to PCP use resulting in violence. Among the Cognoscenti in Miami and the Cools in Philadelphia, fighting was not highly regarded behavior. With or without PCP, members of these groups did not participate in violence and generally were surprised that PCP had developed a reputation as a drug of violence. In Chicago, there tended to be a clearer recognition of PCP's potential of triggering aggressive behavior. The circumstances of violence that were described hardly ever involved viciousness, or were even remotely connected to the kind of bizarre stories reported periodically in the press. Even when the PCP user was designated to be the aggressor, the ability to fight was so seriously impaired by the anesthetic effects of the drug that the outcome was seldom one in which serious injury was done to anyone. One user seemed to sum up the general view when he said, "I have a hard time walking downstairs, let alone fighting."

PCP was associated with violence when users, having had a bad experience, were restrained as part of an attempt to help them. Where these violent episodes took place, they almost always involved a representative of law enforcement or some treatment agent. No effort in our study was made to trace the exact development of those episodes, but the reports from the few users who were the central figures in them indicate that the attempt to restrain them triggered a panic reaction, which in turn was met with greater force until the user was physically subdued. This type of violence tended to be reactive rather than initiated by a user intent on injuring other people.

Most of the violence that was either reported or witnessed was directed not toward people but toward property. In Miami, one user punched a fire extinguisher. In Seattle, Jennifer James witnessed a man attempting to destroy the furniture in his apartment. In almost all cases, the person involved in the PCP-connected violence had already developed a reputation for violence independent of PCP use.

PUBLIC EDUCATION AND DRUG ABUSE PREVENTION

If the groups studied in our four cities are typical, then the efforts of drug education and prevention programs do not reach the street level. Whether those operating the programs lack the ability to penetrate drug-involved groups or whether the groups themselves erect barriers that make penetration impossible is a question that should be explored. Our study indicates that the gap between the two systems was wide and that young people had little interest in what the legitimate world had to say about illicit drugs. Public school drug education programs were largely ignored. Young people disdained them, saw them as "un-hip," and generally resented their efforts to promote total abstinence. Users generally looked on any information associated with public school with disbelief and did not expect to receive honest information from school authorities. For almost all the respondents, prevention efforts were so remote from their lives that no mention was made of them.

The exception to the general observation that young people were untouched by present prevention methods were those programs with an outreach effort such as Upfront in Miami and Northwest Youth Outreach in Chicago. Both of these programs had developed reputations as allies who tried to impart accurate information on drugs rather than as enemies attempting to propagandize young people into abstinence.

Public education efforts, such as newspaper stories and television specials, were both amusing and dismaying to users. In most of them, particularly the 60 Minutes special, which many PCP users in our study saw, the general view was that the exaggeration was so extreme that it did not reflect the nature of the drug experience as they knew it. On the other hand, the recent publicity

on PCP that showed it as quite powerful enhanced user interest in it because the presentation was supportive of the daring and risk that motivated them to try the drug in the first place.

In most cases, however, users thought these public efforts did little to bring about an understanding of PCP or the people who use it because the distortions were so great. One Miami user who knew PCP by the name of "tic" saw and read several of the accounts of PCP and decided that it was a drug she did not want to try. One day, as she was reading a draft of Dr. Cleckner's ethnographic paper on PCP, she was startled to find out that tic, the drug she had been taking and enjoying without too many ill effects, was the PCP she had been warned against in the media. In such situations where the facts about the drug have been so grossly distorted that users cannot associate them with their own experience, distrust of all public education efforts was reinforced.

Even though our study has not yet had much effect on the development of public policy or been included in the kind of planning SSAs are mandated to do, I think you can see how the results of this kind of ethnographic research provide a far different picture of the PCP problem than that provided by other sources more distant from the noninstitutional population. We believe this perspective is essential to planning intervention, education, and prevention programs, and that it has the potential to be helpful in shaping public policies that are both realistic and human.

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Problems Utilizing Ethnography in a Single State Agency

Edward Preble, M.A.

The main problem in using ethnography in a Single State Agency (SSA), or within any agency is that there are no ethnographers there. In fact, there are few ethnographers anywhere, and they are either on reservations looking for subjects more native in appearance than they are, or they are in this room.

Ethnographers, in the traditional sense are either dead or too old to get out the tent door. By traditional ethnographer I refer to any textbook account, such as Herskowits':

> To carry on his fieldwork, he goes to the people he has selected to study, listening to their conversation, visiting their homes, attending their rites, observing their customary behavior, questioning them about their traditions as he probes their way of life to attain a rounded view of their culture or to analyze some special aspect of it. . . . The success of his work, in very great measure, depends on his sensitivity to the situations he encounters, on the interplay between his personality and the personalities of the natives with whom he must deal, rather than his skill in manipulating test tubes or balances or incubators.
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> (Herskowits 1947)

Herskowits was defining the doctrines and methods formally introduced by Malinowski when, by the accident of World War I, he

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became a captive participant observer in the Trobriand Islands. From his experience during this friendly internment, Malinowski transformed ethnography from a cultural mapping-out (graphing) procedure to a sociodynamic study of human behavior.

Although Malinowski formalized and popularized this observational style of ethnography, it was practiced as much as 2,200 years ago by Herodotus. His main achievement was the chronicling of the Persian Wars, but he also paid attention to events and conditions off to the side. He would go down to the "common stews"--an area in a city equivalent to New York's Time Square/8th Avenue area--in order to find out what was going on there with the ordinary citizens.

He discovered, for example, that in the common stews were undercover representatives of the king who had been sent to get information on seditious plans and activities. Often the best informant was an attractive daughter who sold her sexual favors there and, incidentally, got information about any plots against her father. Herodotus reported that one such daughter required, in addition to her usual fee, that a customer bring to each appointment one block of stone which would be saved for her tomb. He said that when she died a tomb was erected which was 150 feet square at its base. She obviously had a long and prosperous career as an informant for the king. This is only one of many examples of peripheral information that Herodotus recorded along with the chronicling of great events such as battles and the succession of royal titles (Goldolphine 1942).

Like Herodotus, all anthropologists who work as participant-observers have one thing in common--they are nosey busybodies who keep their heads on a swivel looking for the unusual and unexpected. It is this attention and interest that Bruce Johnson has referred to in his paper as "the pursuit of the tangential." E.B. Tylor, the founder of cultural anthropology, called this activity "the pursuit of leftovers," and thought well enough of it to define cultural anthropology as "the science of leftovers." Freud, too, became involved in tangentials, which led to his discovery of free association as a revolutionary method for studying the psychodynamics of human behavior.

While referring to Freud, this is a good place to state that I first interpreted the tensions developing between my colleague Bruce Johnson and me, while working together in an SSA, as the consequence of an Oedipal conflict--our having the proper age differential. That was a mistake. The tension was more the result of a generation gap.

Graduate school scholars emerging after 1960 were raised in the computer culture of the mid-20th century. Provided with computer hardware and the latest edition of the SPSS, they have developed an immunity to the inferiority attacks many of their predecessors in the 19th and early 20th centuries suffered when attempting to achieve for their studies of human phenomena the scientific status won by mathmeticians in the 17th century and physicists in the

18th century in their studies of natural phenomena. In attempting to adopt the conceptual and methodological canons of the mathematical and physical sciences and reduce explanation to generalizations in the form of laws, theories, or statistical regularities, many were frustrated and apologetic. Some, fortunately, were not intimidated and discouraged by the charge of being "unscientific." Lasting studies of human behavior--Wealth of Nations, Democracy in America, League of the Iroquois, Das Kapital, Division of Labor, The Golden Bough, Interpretation of Dreams, The Protestant Ethnic, Folkways, Middletown, Street Corner Society--were created without the use of questionnaires, control groups, computers, and printouts.

I know of no studies conducted with the use of such artifacts that belong on such a list. As to credibility without the support of modern statistical techniques, Lewis H. Morgan found it sufficient to say in the introduction to League of the Iroquois, "The credibility of a witness is known to depend chiefly upon his means of knowledge"; and then states, "Circumstances brought me in frequent intercourse with the descendents of the Iroquois, and led to my adoption as a Seneca" (Morgan 1951).

This is all he said about his means of knowledge, and it was enough.

Of course, the pursuit of the tangential--a phrase I am beginning to like--can be reduced to the absurd when it is so undisciplined as to be scatterbrained or aimless. But then statistical procedures and other rigorous scientific excercises can also become absurd. In a New York State agency, I heard a conversation about 10 years ago in which one researcher said seriously to another, "What we need in this project is an instrument to validate the instrument."

Students of human, cultural, and social phenomena will always be vulnerable to the charge of being unscientific. Last year I was in a Seattle bookstore where scientific books on such subjects as mathematics, physics, chemistry, and biology are shelved under the proper categories. When I asked the bookseller where the social science books were he pointed and said, "Over there, with the mysteries." Flesh and blood phenomena always pose mysteries, for which we may be thankful. Human beings are not balls rolling down an inclined plane, subject to Galilean measurement and prediction. This does not mean that students of such phenomena cannot be true to the ideal of science, which, in the barest and truest sense, is a generality of statement without being so vague as to lose any specific relevance, formulated in such a way to have the widest application possible. Reduced to the simplest terms, the methodology for arriving at such statements is to look carefully at what is going on, using whatever means of observation are appropriate and available--whether they are questionnaires or a storefront stoop.

If my remarks here favor the qualitative or naturalistic approach, it is because that is what I practice. Also, it is the underdog in

competition with the quantitative approach. To illustrate this underdog status I can cite an experience I had recently when appearing in a New York State Supreme Court on behalf of a research subject facing sentencing for attempted murder. Before I made my "in mitigation" statement, the judge asked me to describe my work; I gave a brief account of the activities of a research scientist in street ethnography, to which he responded, "Oh, you conduct statistical surveys." At this point, I lost my courtroom composure and exclaimed, "My God, no, I fight with those number crunchers." He laughed, more at my indignation than at the substance of my response, and said "I'm sorry, I understood you to say that you were a research scientist." When research is equated only with statistical surveys by a presumably sophisticated person, we naturalists have to fight.

This battle is not new for me. My first encounter came about 20 years ago when I applied for a National Institute of Mental Health grant for the purpose of studying what I observed in my early work on the streets of New York as a movement by ghetto youths away from street-gang fighting to heroin use. The street-gang problem was getting great publicity in the media and attention in the research and treatment sectors of the social and psychological sciences, while heroin use was receiving no attention or publicity on any of these fronts--even from those agencies responsible for monitoring street-youth activities at the time. I began to observe this change from street-gang fighting to heroin use on the streets as director of New York City Evening Community Centers in Yorkville and East Harlem, even though these events were tangential to my assigned job of conducting volleyball games and square dancing. This practical work in tenement areas of New York and my academic work in cultural anthropology complemented each other, and I proposed in the grant application to use the experience and knowledge from both areas of experience.

I knew no one in the Federal grant field at the time and certainly no one knew me. I went through the usual application procedures, following the book of intructions line by line and sent it in without consulting anyone at NIMH beforehand to find out where the research gaps were, as we do today. I did not even know where NIMH was located and simply mailed the application in the return envelope that was provided. My application was rejected out of hand, without even a site visit. The only criticism that was given was that it did not deal with large enough numbers and had no control groups; in short, it was--in the exact words of one reviewer, a well-known epidemiologist--"merely a naturalistic study."

At this innocent stage of my career it struck me as an outrageous irony that something was considered invalid on its face because it was natural. In the rejection notice there was the routine statement that I could reapply using the offered critique to guide me. I did reapply, but instead of making it quantitative, I made it more naturalistic and vented some sarcastic humor. This was in the nature of Galgenhumor, because I did not expect any positive response, if any response at all.

But, fortunately, there is a Morris Schwartz. He and Alfred Stanton had published in 1954 the famous study, Mental Hospital; it involved a sociologist and psychiatrist collaborating in a pioneer study of a mental hospital (Stanton and Schwartz 1954). Schwartz was on the NIMH review committee that reviewed my second application. Something about the application attracted him and he persuaded his colleagues to allow a site visit, with him as one of the site visitors. He came, along with a psychiatrist who was interested in studying the natural psychotherapeutic personalities of bartenders, and had calculated that New York was a good place to further his knowledge, which, believe me, he did. Schwartz thought that I had a good idea; the psychiatrist could not have cared less.

Although Schwartz had a positive interest in the application, he was pessimistic about my success in getting the grant award because of the quantitative issue. However, in driving from one bar to the next--for the benefit of his colleague--I spotted on the street a young, 16-year-old Irish-American boy I knew from my work, who was a heroin addict. Remember, this was 20 years ago, and was therefore unusual. It is hard to realize this in 1979, when drug use is so common and public; then, a young heroin addict was as uncommon as a boy scout is today. I stopped the car, invited the boy in and we rode around and talked about street-gang fighting (he had been a gang leader) and the spread of heroin use on the streets. I knew him quite well and he spoke freely to all three of us. Schwartz was impressed after this encounter and said he would try his best for the grant. As a result, I got it, and for a substantial, 3-year period. For me, it was the sweetest irony that one individual (the 16-year-old boy walking down the street) outnumbered, in effect, all the quantitative-minded critics.

That NIMH study of the transition in New York City from street-gang fighting to heroin use among street youths was completed in 1962 and parlayed into some further studies and consultation work during the next 5 years, with journeyman results. One of the interests that emerged for me somewhere along the line was in the economic structure of the heroin market, and I casually mentioned this interest to others. One day John Casey, a young economist, called and said that he would be interested in working with me on a formal economic description and analysis of the street heroin market in New York. We agreed to collaborate, and I supplied him with cartons full of life-history interviews, field journals, observations, and anecdotes that I had collected and saved over the past 10 years.

After reading through this material and taking notes and excerpts, we wrote the draft of an economic paper. The burden of the article was an economic interpretation, using modern economic theories and models, expressed in the technical language, tables, and charts of economics. My contribution to that draft was a throwaway summary at the end where I supplied a general cultural background for the economic exposition.

When we read through the completed draft, we were not satisfied. The data was good, the analysis was logical, and the charts and tables were attractive. What was missing was something interesting and important. We could picture a reader nodding in assent after reading it and saying, "Yes, that's probably true--so what to you want from me?"

In attempting to liven up the article, we juggled sections around and finally using this hit-and-miss method, placed the summary at the beginning of the paper and guided it through a natural development to a satisfying conclusion. The result was "Taking Care of Business," a paper that has kept me in business ever since (Preble and Casey 1969).

At the risk of appearing self-serving, it may be instructive to compare the academic acceptance of that article with its use by State and other policymaking bodies--the latter being the topical concern of this symposium. Briefly summarized, "Taking Care of Business," criticized the long-time acceptance of the medical-psychological model that associates opiate use solely with physiological and psychological needs. This model was criticized in the paper as being too narrow and not taking into account sociological factors related to opiate (primarily heroin) use, particularly among lower-class minority groups. The paper concluded that, "The career of the heroin user serves a dual purpose for the slum inhabitant; it enables him to escape, not from purposeful activity, but from the monotony of an existence severely limited by social constraints, and at the same time it provides a way for him to gain revenge on society for the injustices and deprivation he has experienced."

The data and findings of this paper were generally accepted by students and researchers in the field as calling attention to neglected aspects of the drug-use causality question. The findings have been favorably cited and incorporated in the drug use literature during the past 10 years. However, in spite of academic acceptance, this paper and similar works of others have had little influence at the practical, policymaking levels of drug prevention, treatment, controls, and legislation.

One of the heartening exceptions to this rule that I can report occurred when the former Commissioner of the New York State agency concerned with drug abuse, Daniel Klepak, invited me to talk about our work at a meeting of his top administrators and policymakers, and later joined us in rounds of street addict hangouts in New York City. His favorable reports on this experience to members of his staff led to productive exchanges at several levels within the agency. The fact that Klepak's successor, Julio Martinez, is a Puerto Rican ex-addict from the streets of East Harlem is cause for further optimism among street ethnographers.

One reason for the paucity of ethnographic studies of street drug use, in addition to the lack of demand, is fear on the part of potential researchers of: physical harm (exaggerated);

involvement with law enforcement agents (exaggerated); and dirt and discomfort (not exaggerated). Partially due to these fears, researchers frequently employ surrogates, mainly former heroin users now on methadone maintenance programs--euphemistically called indigenous workers. Although useful in certain ways, the ex-addict in drug abuse research and indigenous workers in other research studies frequently constitute another level separating the professional researcher from his subjects.

There are ethnographic researchers, however, who are not afraid of the streets, do not delegate direct contact solely to indigenous workers, and get out there; they are probably all in this room. The impact of their reported work at the State policymaking level has been almost negligible. Although I am not involved in shaping policy decisions, I have a layman's knowledge that State personnel implement decisions of those above them, including the policymaking head of the agency. He, in turn, is guided by the political realities of the day, which ideally are realities of the citizen's everyday life. This is, of course, the way representative government functions. Breakdowns in communication and problems that occur in the policymaking process may be due to inadequate, mistaken, tardy, or unreported information. An illustration from the early street-gangs study, referred to earlier, may serve to illustrate the potential of ethnography to help remedy these faults. This example is far enough in the past to be noncontroversial.

From 1955 to 1960, little was known about drug use on the street. Heroin use developed rapidly after 1950. By 1955, heroin addiction among youths was apparent to those living and working in slum neighborhoods. By 1956, the use of heroin among street youths had destroyed the fighting street-gang culture. The New York City Youth Board and other public and private agencies had "detached workers" assigned to fighting street gangs since the early 1940s, and their work had been dramatically publicized and rewarded by substantial financial support.

During the period from 1955 to 1960, heroin use and associated psychosocial problems were ignored by these youth service agencies. They continued to devote their resources to the now mythical street gangs. This was true in Manhattan neighborhoods and in other areas of the city. I worked as a part-time consultant to the Youth Board in 1960 with a South Bronx unit ostensibly working with street gangs. The unit had rosters of supposedly active gangs that had terrorized the neighborhoods. When I went out to observe these fighting gangs, I could not find any--even with the help of the workers designated to care for and observe them. Young heroin users, however, were easily seen nodding on street corners and in candy stores, and hustling the street in between these times. One day I asked a worker, "Where does your office get those rosters?" He was a candid person with whom I had developed a good relationship; he said, "Come on, I will show you." He took me to a schoolyard where some teenage boys were playing basketball, called them over and said, "Do you want to get down on a good thing? We got bus trips to Coney

Island, ball games, picnics, and have a good time." The response was, "Yeah, great--sign us up"; the worker got about 30 signatures. This became a "street gang" in the unit office file in the Bronx. I believe you could find "Doctor J" (Julius Irving) on one of those old youth board rosters.

There was, however, some hard evidence about heroin use that could not be ignored forever. In the mid-1950s, Bellevue Hospital started getting admissions for heroin use of young people turning themselves in or being brought in by police, other agencies, and parents. They came in with overdoses, for detoxification, and for other disorders associated with heroin use--such as serum hepatitis. By 1960, 5 years after heroin use had become a widespread phenomenon, State and city agencies went into action. The State attempted a transition of interest and action from street gang fighting to heroin use by appointing a former youth board administrator to a top position in the new State narcotics agency.

In 1961, President Kennedy called a White House Conference on narcotics in Washington. It was an ironic coincidence that news of the ill-fated Bay of Pigs invasion was delivered to President Kennedy on the stage while he was addressing this conference--government actions in both areas having been based on faulty intelligence reports. Nevertheless, that White House Conference initiated a rigorous, if belated, attack on the problem of narcotic addiction that continues in force today.

It is of historical interest to note that the only narcotic treatment group organized at the time of this conference was Synanon. Their members were not invited to the conference, but they had rooms in a hotel where they attracted conference members and other interested visitors through hand-bill advertising on the streets. Synanon went on, of course, to become the progenitor of drug-free treatment programs, encounter and consciousness-raising groups, and, recently, mountain minutemen. But that is another story.

Two-thirds of the dialectic that Johnson and I pursued in our collaborative work in a State agency has been sketched here so far; it remains to complete it with a preview of the synthesis we hope to achieve.

Certainly both the quantitative and naturalistic methods can contribute to the body of knowledge about drug use that we all strive to build. In our project, we are combining these methods as we proceed with the work, rather than pursuing a polemical or merely juxtaposed use of approaches often thought to be incompatible. An example here from our current work will serve to illustrate how we are utilizing this combined approach.

We have been using a structured interview guide with 40 research subjects regarding their economic behavior over 30 consecutive days. The major categories of inquiry are:

- Employment
- Criminal activity
- Drug use
- Treatment
- Income
- Expenditure

When tabulated, the results provide a more accurate accounting of the economic life of street drug users than do memories. On the other hand, the nature and structure of questionnaire interviewing inhibits spontaneity and elaboration that can contribute additional relevant and supportive data. One subject, a professional "stick-up-artist" and "hit man," expressed the problem this way: "You can't learn anything from me by just letting me fill out a questionnaire; that's too cold and rigid. You could be giving the right answer, not lying, but not have your heart in it. Like an architect, he designs a building so when the wind blows it won't break, it has some sway in it in the right direction. A questionnaire doesn't allow for that, it doesn't fill in the swing and sway. But when we talk free for 10 or 15 minutes before and after the questions, like here, we get some sway; like the building, you get good answers all the way through."

This subject's appraisal is reinforced and illustrated by the questionnaire responses and elaborations on them by two other subjects--a pimp and his number-one girl (Kenny G. and Cathy D.)--who reported their economic activities for one day in successive, private interviews.

A separate 15- to 20-minute period of elaboration by each Kenny G. and Cathy D., provided information that was summarized in the research notes, as follows:

Cathy and Kenny came together as usual. Cathy was interviewed first while Kenny waited in the other room.

In responding to questionnaire questions we got to "robbery," and Cathy related the following: Yesterday Kenny asked her to go downtown and turn a few tricks because his main money-making girl had been locked up. She reluctantly agreed. On the corner she was approached by a white man in his middle forties. She agreed and they went into the hallway, toward the back of a nearby building. As he was undressing Kenny appeared from the backyard entrance with a pistol, took about $240 and credit cards from the man and forced him to undress completely; then he and Cathy left. Kenny hailed a cab for them, went a few blocks, and then told her to get out and go home, without giving her any money.

Regarding the "treatment" question on the questionnaire, I jokingly asked her if she had received any treatment beyond what Kenny had given her. She laughed and, when asked to characterize that treatment, said, "Bad treatment."

Kenny came in after her and related the following: One of his girls had come in the morning and "laid some scratch on me" ($125). Later, she called saying she had been arrested for "prossing" and they were holding her until some warrants on her were raised. She asked him to bail her out. Inside of trying to do that he spent the $125 she had given him on coke for himself and then directed Cathy to turn some tricks for him in order to recoup the money he had spent on coke. He said he usually doesn't ask Cathy to do this, but now he needed money because his main girl was out of action. Cathy objected, but he said, "Come on, let's go," and she complied. It was his original intention to have her turn four or five tricks and get the money he needed, but he got impatient and decided to take the first trick off because it was faster and might result in more money. He took the risk of the robbery because of the effects of coke. This was not his usual practice. And about Cathy, he said, "She can make more for me boosting than on her back, but I was in a hurry." He described the robbery in the same way Cathy did and said he got $238 and several credit cards. He used one card for two pairs of shoes (cost, $175) and one card for a jacket ($95). He showed the interviewer one pair of the shoes he bought.

He said that he was mad at Cathy because she had resisted the idea and he resented her ingratitude for his having "fit her into my schedule".

About the prostitute who had given him the money and got arrested, he said that she has warrants on her for robbery all the time because she likes robbing a trick with the help of an accomplice, as he and Cathy had done.

When collected and statistically processed along with hundreds of other interviews from different subjects, the questionnaire responses of Kenny and Cathy contribute to meaningful printouts that can be analyzed in many ways. The elaborations, however, cannot be punched out on IBM cards; they must be incorporated in a more naturalistic format that both utilizes and serves the quantitative analysis. If this different procedure offends purists in either methodological camp, or they simply do not get it--one can only say, with G. K. Chesterton, "I don't write for people who don't get it."

Having made this rather smug declaration, let me redress matters by saying that one of the dangers street ethnographers have to guard against is smugness. The fact that one is on the street directly observing and interacting with those who, perhaps to his envy, seem to be enjoying the expression of socially forbidden impulses, is not a guarantee of accurate description, analysis, and explanation. The early Greek philosopher, Thales, is reported

to have said of certain of his contemporaries "Present, they are absent"; and that can apply to ethnographers as well.

The converse is not necessarily true. One can become an expert on street drug use without leaving Montana; or he can be a State agency administrator. Sir James Frazer never left the libraries of Cambridge University during his 50-year anthropological odyssey. And no one claims that you must have participated in the Civil War to qualify as an expert on the sociology of that period. The case can even be made that distance in time or place contributes to a desirable objectivity in the study of human affairs, but that is a case for someone else to make.

With libraries, current journals, news media, colleagues (including street ethnographers), and a creative commitment to the study of social problems, no one is disqualified for that study in advance. It is encouraging to observe at this symposium impressive evidence of such a commitment.

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Ethnographic Research and Public Policy Development

Jennifer James, Ph.D.

Researchers in the social sciences are having a significant and expanding effect on public policy. There is increased concern in the United States about social problems that affect the public welfare or quality of life. Criminal activity, sexual violence, drug abuse, street gangs, and child abuse are just some of the areas concerning public officials. Legislators and agency administrators are seeking information about these problems so that their decisionmaking procedures can be realistic. Social scientists have become the "experts" providing advice to policymakers. The interaction between social scientists and public policy agencies has not, however, always been successful. There has been confusion between social "theory" and social "reality." Many researchers do not work in the field, do not in fact "know" their subjects as individuals nor make themselves aware of the environment in which these people live. The ethnographer is developing an edge at this point in research, aware of both the "real" knowledge of the subjects' reality and the "ideal" knowledge of the social scientists.

Ethnographers are concerned with the subjective perceptions of their informants as well as objective measurement. They are perceived as researchers who know the population about which they are reporting. Such insight is of increasing interest in the development of public policy. The research goals of the field ethnographer are built around the cultural concept of value as well as behavior. These goals require first-hand knowledge of a social group, village, or subculture.

During the past 10 years we have applied the insight gained through ethnographic observation to the problem of prostitution. We have found that policymakers quickly perceive the quality of such data. Our experience in shaping public policy toward prostitution illustrates the potential influence of ethnographic data. This practical experience also clarifies the possible benefits as well as problems faced by social scientists who take part in public decisionmaking processes.

PROSTITUTION AND POLICY DEVELOPMENT

Prostitution is a classic public policy area for ethnography because of the emotion that surrounds it. Legislators, police officials, court personnel, and social service staff come to their occupations with a set of beliefs about prostitution based on their cultural values. Various segments of the nonofficial public have other sets of beliefs. Virtually none of the subsequent recommendations or opinions are based on first-hand knowledge of the activity. Prostitution involves sex, sex involves morality, and morality often defies logic. Beliefs about prostitution are emotional because of their basis in sexual ethics, traditional values, limited information, or misinformation.

Most social scientists who have studied prostitution have reinforced the emotional chaos that pervades policy discussions. Publications center on prostitutes who were mental health patients (Greenwald 1958; 1970), imprisoned (Bewley 1967; Choicy 1965), reformed (Slim 1967), or derelict (Murtagh and Harris 1957). Charles Winick (1971) based his data on second-party reports of what prostitutes had said or done. Generally, professional social scientists stayed away from the subject because it did not lend itself to precise measurement or academic status. The information available to policy makers was, and is, based primarily on "exposes" by journalists (Stern 1956; Sheehy 1974).

Street prostitution represents an identifiable subculture for the ethnographer interested in social change. The group is visible, boundaries can be perceived, and the "reality" is essentially unknown. Prostitution is increasing, the public is ambivalent, and lawmakers are attempting to reclassify its criminal content. Prostitution is viewed by some as a crime without a complainant--or a "victimless crime"; others view it as a magnet for serious street assaults and robberies. It is, in fact, increasingly becoming both, but the significant lack of information surrounding it hampers appropriate social and enforcement action.

The study of street prostitution requires participant observation. It requires real-world validity and experience. I found it a classic "urban laboratory" for the tools of an ethnographer. Prostitutes who work the street have shared values and beliefs, as well as a unique language and social system. My initial interest in prostitution grew out of an interest in the argot of streetwalkers, how their language molds perception. This project eventually became a dissertation (1972). My interest in the subculture of streetwalkers was enhanced by the continuing public policy debate in the city in which I lived over the development of more restrictive laws against prostitution. The data gathered during my first 2 years of field research, when translated into public testimony, became an important factor in subsequent policy development. The subsequent 8 years of research have made this field research a common resource for policy staff.

ETHNOGRAPHIC DATA AND PUBLIC DEBATE

Knowledge of a subculture such as that of streetwalkers, previously spoken of only by police, pushes the ethnographer into the public arena. I was able to provide both qualitative and quantitative data to various legislators and policy hearings. I was able to provide logical, understandable, in-depth information that frequently contradicted the police point of view. I was able to establish credibility because I, like the police, was experienced. I actually knew and had spent time with prostitutes, not just one but more than 100, during an extended period of time. The result was the opening of a debate within policy and public decisionmaking circles. I was careful throughout these discussions to maintain a balance between academic credibility and ethnographic experience.

The open debate within the community over prostitution and enforcement policies resulted in considerable change. There were rapid changes in policy enforcement procedures, sentencing, bail and appeal bonds, and the jail environment. Eventually the legal code was revised and overall city policy evaluated.

At the time of my research, the prostitutes pointed out that the vice squad were disrobing in order to make a prostitution arrest, and in some cases completing sex acts. Civilians were also hired by the police department to act as decoys and frequently completed sex acts. The exposure of the behavior resulted in new policy orders from the city administration. The police were no longer allowed to take off all their clothes; they had to keep at least undershorts on. The police and civilian agents were restricted from engaging in direct sexual activity. The result of these changes was a reduction in "pay-offs" by prostitutes to police and an upgrading of enforcement priorities.

Sentencing procedures, bail requirements, and appeal bonds were also examined during this research. The prostitutes claimed they were discriminated against in court. In fact, compared to other misdemeanor arrests they were sentenced for longer periods, charged twice as much bail, denied personal recognizance, and charged exhorbitant appeal bonds. The justification for this was based, not on public policy concerns, but on police pressure on the court personnel. Exposure of this discrimination resulted in reduced sentences, bail, and appeal bonds, and in equal eligibility for personal release.

The women's jail was the third area of public policy debate. More than 50 percent of the population were prostitutes and the environment was far below standard. The women were not allowed certain amenities--i.e., hair color, tampax, douches--because the male jail sergeant didn't approve. Streetwalkers spend a lot of time in jail and it was a frequent topic of conversation. The prostitutes were referred to as "girls," treated like children, and dressed in uniforms with short skirts. They were given no access to education or training. Health care was minimal and, in

the case of pregnant women, inadequate. Public debate, when it included the prostitutes' perspective, resulted in improvement. The end result was a jail environment that provided a slightly increased possibility of diversion off the streets.

Court suits were instituted by the American Civil Liberties Union, and the public defender challenged the entire spectrum of prostitution laws. This required the rewriting of statutes and the opening of a public debate on police priorities and decriminalization. A court order on discrimination resulted in a requirement for the equal arrest of customers (City of Seattle v. Jackson, Crim. No. 69171 [Superior Court] 1975). Organizations were formed, such as COYOTE, to change the laws; and national groups such as The National Organization for Women and the American Bar Association passed resolutions. Bills supporting the decriminalization of prostitution were introduced on the local, State, and national level.

The prostitutes not only talked about unfair legal and enforcement procedures, they were also concerned about juveniles. Customers often asked them to find younger girls (e.g., under 16) and the women thought this was wrong. The result was increased public awareness of the special problems in juvenile prostitution and the beginning of a revision of enforcement priorities.

Another area exposed by the streetwalkers was the refusal of police and prosecutors to deal with abuse of prostitutes. Rape, domestic violence, and assaults were ignored because the police did not want to arrest customers, and the prosecutors found pimps hard to convict. Enforcement procedures concentrated on the least dangerous client--the female streetwalker.

All of these issues led to a clarification of real community concerns and needs in the area of prostitution laws. The public was somewhat in agreement with the prostitutes' perception of their profession. The real issues were juvenile involvement, associated crime, public affront, and public health. Discussion of these issues made the process of handling prostitution a more practical one. Personal morality was replaced as an issue by actual community needs.

The debate over prostitution contributed also to an expansion of research support and, therefore, available information. Many contributions were made to the prostitution literature outside of anthropology, but the core rethinking of the issue was tied to the ethnographic issues of the cultural reality of street prostitution. In subsequent ethnographic work these studies centered on customers, pimps, police, male prostitutes, and juvenile prostitutes. The utility of ethnographic data for the continuing debate over prostitution is clear. Careful ethnography can describe prostitution as the streetwalkers themselves see it and contrast their reality with that of the policymakers. The discrepancies between the two conflicting viewpoints provide an opening for public debate on the validity of either. Such debate results in policy revisions based on community realities and needs that are historically the primary basis for morality laws (James 1976).

CONCERNS AND PROCEDURES OF ETHNOGRAPHIC RESEARCH

The interest of the ethnographer in this policy debate is the ultimate resolution of the conflicting viewpoints surrounding prostitution or any other social problem. Therefore, one must be concerned first with the accuracy, ethics, and scientific and social goals of the study. Ethnography is potentially a powerful contribution to social change because of its insight into different cultural realities. Such information must, however, be carefully gathered and reported to provide a real contribution to public policy development. Judgments made on the basis of inaccurate research will rarely provide successful solutions to human and social problems. Considerations of social goals and responsibility for results are crucial in the application of ethnography to social problems.

The success of our project's input into public policy development was a result of our awareness of the possibility of ethnography and the potential problems of applying it to prostitution research. The implementation of an urban ethnographic project requires consideration of the urban environment, varying cultural realities and accuracy, procedural restrictions, contact situations, informant-researcher relationships, and public access to results.

ACCURACY AND CULTURAL REALITY

An example of this situation is the view of the jail held by many informants, as opposed to other views of the jail held by the police and city administration. Many informants had been prisoners in more than one jail and often gave general replies in response to questions regarding the jail in which they were currently incarcerated. Thus, a statement regarding the physical layout of a jail may be accurate in terms of the informants' perceptions and conceptualizations of "jails," but not in terms of the jail where they are at the time of questioning. The same discrepancies can occur with respect to jail personnel, incidents that occur (e.g., theft of personal property), adequacy of medical care, etc. Publication of this general "cultural reality" of the informant, which has been a problem in previous studies, may be perceived as irresponsible by agencies that think in terms of their own "cultural reality," which is confined to a single jail. The misunderstandings and charges of inaccuracy that can result are obvious. Social scientists frequently concentrate on the "research problems," not the larger context of social research. The ethnographer cannot afford to do this in research with policy implications.

Also relevant to the issue of different "cultural realities" clouding the assessment of accuracy is the problem of the pluralistic nature of the urban situation (Spradley 1968), especially when one is dealing with activities defined by the groups in power as illegal. Although it is impossible to describe a situation as viewed by all

possible participants, some attempt must be made to at least acknowledge the differing perspectives. For example, the perspective of the prostitute conflicts at many points with that of the vice squad officer. An example of such conflict is the question of "police brutality" vs. "self-defense." Vice squad officers maintain that they are kicked, knifed, and abused by prostitutes, and that any seeming brutality on their part is self-defense. Few women, when specifically questioned, said they had been roughly handled by police, yet most stated on a general level that police beat up other girls or forced them to make sexual payoffs. Detailed research and observation revealed that informants had been physically abused by the vice squad but usually in the context of resisting arrest. There are serious problems in the strength differential between police officers and women. The resulting abuse is sometimes intential, but it is not common. Other groups in the population, such as those who read sketchy accounts in newspapers, will have their own views of whether a given incident is a case of "brutality" or "self-defense."

Psychological harassment by the police is a much more common problem and is, in many senses, more abusive. Women are called names, taken for rides in police cars and left to walk home, or held in the vice squad office for questioning. The police may deny this occurs, but in fact observation confirms its frequency. Conflicts such as this are ubiquitous in pluralistic urban environments, and such conflicts must be carefully researched and described for the qualifications of research results and made clear to potential users of the data. Unless such efforts to evaluate qualitative data objectively are maintained, research will contribute little either to understanding or to effecting change.

PROCEDURAL DEFINITIONS AND RESTRICTIONS

Procedural definitions and restrictions in urban policy-related research are also important. In one's own society value imposition results from familiarity with the social problem of prostitution. There were two extremes to this research concern. One was the cultural notion that prostitutes are morally deviant, maladjusted women. This assumption may be seen as related to a double standard in American morality that condemns women who engage in sex for money, but not the men who pay them. This moral bias has permeated most of the literature on prostitutes in the United States, hinders objective analysis of the phenomenon of prostitution, and results in prejudicial handling of prostitutes by the larger society, most particularly by law enforcement agencies and judicial personnel. At the other extreme is the attitude of some who have been called "bleeding-heart liberals" whose value imposition takes the form of whole-hearted acceptance of the prostitute and her profession, with accompanying hostility toward the authorities and the value judgments of the larger society. Just as the assumptions of the "moralist" have produced biased research, so has the approach of the "liberal." Both attitudes represent evaluations made prior to data collection and analysis that decrease the potential utility of the research. On the

positive side, awareness of these biases in the literature and among the larger population with which we interact makes ethnographers more sensitive to the possibility of their own biases, and thus serves as a restraining influence.

Translation competence is a similar, yet distinct communication problem also encountered when working with a group within one's own culture. The informants know the researcher's world and deal with questions accordingly. With prostitutes this problem is compounded by the nature of the "hustling" business. For example a prostitute depends for her success and survival on her ability to judge people and handle them, to give the customer what he wants while not doing more than she wants, thus selling her ability to make the man feel he is satisfied. "Conning" is an art of the profession and the researcher is as likely a victim as the customer.

Another aspect of translation competence in this type of urban research is the researcher's tendency to think that he is getting better data than is actually the case because of the seeming cooperation of the informants. This cooperation has many dimensions and must not dissuade the researcher from cross-checking information obtained in personal interviews. In addition, he is involved in his own problem of translation competence because he can easily misuse his ability to supply missing information from his knowledge of the system.

Solutions to the problem of translation competence include extensive interviewing in various contexts--such as group discussions, free conversations, and individual interviewing in depth--and using semantic techniques--such as card sorting (Kelly 1955) to eliminate the normal structure of an interviewing situation, thus enabling the researcher to discover and validate certain data. Other possible solutions include cross-checking informant response through available sources--such as institution files, arrest records, and the comments of friends, relatives, and others in the business. But, the most reliable solution is time. In general, the more time spent with informants under varying circumstances, the more reliable is the data gathered. All of these techniques to control for translation problems are part of the repertoire of the urban ethnographer.

Another problem that has emerged as one of general concern to anthropology in urban research procedure is the definition of social unit boundaries (Spradley 1968; Cohen 1969; Barth 1969; Molohon 1979). It arises in the study of prostitutes because of the cross-cutting networks that are a part of the "fast life." An example that we encounter frequently is the drug addict who works the streets to obtain money for drugs. The prostitutes who do not use "hard" drugs maintain that the addicts represent a distinct subculture. They point out that drug addicts turn to prostitution as a way of supporting their habits but are not otherwise a part of the prostitute subculture. Prostitutes who consider themselves professionals point out that the "hypes" dress sloppily, treat their customers poorly, and are more inclined to

pull "rip-offs" (i.e., steal). Many prostitutes refuse to let addicts become a part of their "stable" (i.e., an arrangement of one male and more than one woman doing business together), and they state that a "good" pimp will kick a girl out if she uses drugs or will try to break her habit; otherwise, the drug becomes the controller, rather than the pimp. The overlapping activities of prostitution and drug addiction make it difficult to separate these components of hustling.

Another aspect of the problem of defining social-unit boundaries is the cross-cutting of cultures or subcultures. Research within subcultures (e.g., that of prostitutes) that are by definition a part of another culture (e.g., the larger "American culture"), as well as other subcultures (e.g., the "black subculture") presents problems of overlapping forms that obscure important cultural differences. This is obvious in research with prostitutes who are members of different racial groups. In attempting to understand the prostitute lifestyle, racial differences form a crucial set of variables. Although the women live and operate in similar environments, sharing the same customers and pimps, they are different in many ways. Black prostitutes must be considered in terms of the black community and subculture that accepts hustling as legitimate in varying degrees, just as white prostitutes must be considered in terms of the larger white subculture that views overt hustling as deviant in varying degrees. Any discussion of deviance and public policy in prostitution must take cognizance of these differences in subcultural backgrounds. The researcher is faced with differences of race within the subculture of prostitutes, even though such differences are minimized by the prostitutes themselves.

THE CONTACT SITUATION

There are three aspects of this problem: access to informants, the effects of the situation on the conduct of research, and the effects of the situation on informant response.

In my original study, initial contact was made through the daughter of a friend who was a prostitute but was trying to quit at that time. She lived with the researcher for a few months. In this situation, with the exception of the researcher's roommate, informants were hard to track down, make appointments with, or depend on for continuing interviews. The "fast life," as it is called, indeed moves fast. It is important to the methodology of ethnographic research to maintain a consistent, continuing contact situation. Therefore, the problem rapidly becomes one of access to informants for sufficient periods of time to make interviewing possible. In addition, it soon becomes apparent that street interviewing is difficult. There are problems of personal safety involved in attempting to be "on the block," and this is further complicated by the fact that any attempt at contact during business hours interferes with business and might be offensive to the informants. The most successful technique is to meet a girl for breakfast, at around 4:00 a.m. when business is over, who is

willing to discuss her "take" and her experiences of that night. This interviewing situation, although more successful, does not provide an adequate sample; a girl who works for "her man" (i.e., pimp) is too eager to get home and report her earnings to be interviewed by an anthropologist. It is possible, of course, to achieve contact by entering into the subculture either as a prostitute or as service personnel, such as cocktail waitress, participant in stag shows, dancer, etc. This alternative was rejected on legal, scientific, and personal grounds that will be discussed later.

Another means of contact is through the city jail where all female prisoners from both the city and the county are held for varying periods. This confined situation simplified interviewing and solved the problem of access to informants. The jail is a place of few diversions and so, with rare exceptions, informants were willing to work with the researcher. The number of streetwalkers serving time in the jail is always considerable, due to the strict enforcement tactics employed locally. Women can be arrested for loitering as well as for offering and agreeing to an act of prostitution. Thus, the jail provided access to a sample that appears to be unbiased because all the prostitutes previously contacted outside the jail had spent time in the jail, and few who work the streets avoid arrest.

The jail situation, however, creates additional problems of its own. The environment affects both research procedure and the responses of informants. To conduct research in a jail, one is forced to seek the cooperation of authorities who have a negative view of the streetwalker and, often, a negative view of the academic community. The researcher is subject to the regulations of these authorities in matters of availability of informants, interviewing limitations, and the interviewing environment.

It is difficult to find out who is in jail at any given time, and on what charge. The best sources are usually unofficial and, as a result, some potential informants are overlooked completely. Interviewing limitations include a restriction on the use of tape recorders and time limits of only a few hours each day. The interviewing environment is sometimes directly controlled by hostile personnel, eliminating privacy; and the noise level in a jail can make interviewing almost impossible. Regulations, because of their inherently rigid nature, plus the fact that they fluctuate with changes in personnel, can waste large amounts of research time. For example, in the situation in which our original project was conducted, there were three police chiefs and two jail directors in a period of 6 months, and with each new officer the project had to be outlined and resold in order for the research to continue.

Another problem encountered in attempting to conduct research in a jail is the informant's perception of the researcher as a connection with the outside. In a jail, communication with the outside is the most valuable commodity anyone can offer an inmate. In this context, the researcher is confronted with constant requests to

telephone lawyers, bail bondsmen, relatives, boyfriends, baby-sitters, etc. Requests are also made for money, cigarettes, nylon stockings, shoes, fruit, popcorn, and other goods that are otherwise unavailable. In such a situation, the researcher's purpose is viewed differently by the informants than by the researcher. If careful control is not maintained, the ability to do research is limited because of lack of time and energy after all requests have been filled.

With regard to the effects of the contact situation on informant responses, the jail is a negative environment that affects the objectivity of data collection by changing perceptions. An example is provided by the questions posted during general interviewing that refer to the informant's feelings about herself. When she is incarcerated the answers given may vary from the answers to the same questions when asked outside the jail.

Considering all the problems involved in using the jail as the contact situation, another alternative was sought outside the jail with women who were no longer working as prostitutes. A woman who has spent a few months in jail will sometimes stay off the streets for a while and may be willing to be interviewed. If a good researcher-informant relationship has been established in the jail, it can usually be continued after the informant's release. In this situation the social environment of the informant is more easily understood, and the pressures of survival in that environment become more obvious. The effect of this "in-between" environment on the released but, for the moment, reformed prostitute is similar, though not identical to the one encountered in the jail. It is a period of considerable ambivalence for the informant who lives the "straight" life but maintains her contacts with the "fast life." In this situation the ambivalence is reflected in informant responses and has to be taken into consideration when interpreting data.

The final contact situation possible in this research is retired prostitutes who are available for interviewing. After some attempts in this direction, this approach was rejected because of the need for a limited descriptive time period and also in the interests of accuracy. Older, retired prostitutes report vast changes in the business of prostitution and usually put down "today's whores" as lacking style or sense, often characterizing them as "thugs--not ladies." Interviewing someone, regardless of age, who had been separated from prostitution for 2 years or more produces interesting reference data but questionable information with regard to the present circumstance.

This review of the alternative contact situations employed in street research contains implications for any urban researcher. As indicated in specific contact situations and in relationships with the authorities and informants with whom one is working, the researcher must strike a balance between research objectives, the limitations imposed by authorities, and the needs of informants.

INFORMANT-RESEARCHER RELATIONSHIP

A procedural problem closely related to the contact situation is the informant-researcher relationship. Ethical problems can be intensified when urban researchers find their informants are engaged in activities that involve public policy issues. Thus, in research with prostitutes, one is caught between responsibility toward the informants and responsibility toward the enforcement structures of the larger society. The informants are suspicious of outsiders, and the police are interested in any information gathered by the researcher. The assumption that the researcher's data are his or her own can be unwise. A most obvious case is that of a journalist who has been subpoenaed and legally required to disclose sources of information contained in articles written on topics such as drugs and abortion. Law enforcement's respect for the researcher is not on the same level as respect for the secrecy of the confessional.

The anthropologist must provide for the protection of informants as specific individuals and as members of a subculture. Safeguards employed in this research have included signed agreements with both the institution supporting the research and the enforcement agencies involved, stating that the basic data are controlled by the researcher. More specific safeguards have included coding personal information, deleting names and locations in tape transcriptions, and storing the data in computers, enabling the researcher to destroy the initial questionnaires and notes.

The protection of the informant must subsequently be balanced with public policy considerations that affect informants. Will public statement on police abuse result in increased or decreased danger to prostitutes? Will exposing data in public policy debates improve the circumstances of the street environment or potentially attract attention negative to the prostitute's articulated interests? Resolution of these concerns is important to the informant-researcher relationship and is discussed later in this paper.

A second area of the informant-researcher relationship is regard for the informant as an individual and the possible offensiveness of researching personal matters. This is a difficulty. The lack of sensitivity to such possible offensiveness not only violates the rights of the informant as an individual, but can also close doors to continuing research and to future projects.

Inquiry into prostitution requires personal questions. However, responses are carefully weighed and particular lines of questioning are dropped when they seem to engender stress. This does not affect the results of the research because some informants are more willing to discuss personal matters than others and longevity of research eliminates most stress. Such information can be gathered from informants who do not feel threatened by personal inquiries or in different types of interview situations.

Agreements made in the context of the informant-researcher relationship form the third area of consideration. Human rights

must be understood to extend to informants. Difficulties arise when dealing with one's own culture and center particularly on economic factors. One recurring problem is the matter of informant fees. What is the proper exchange for the informant's time? In the jail, a suitable agreement involved providing the otherwise unavailable services discussed above. However, the researcher is in a privileged position in this matter and must constantly be on guard not to exploit such situations as the jail, where he/she represents a communication with the outside the informant does not wish to lose.

The remaining point is the problem of participant observation in urban research involving illegal or deviant subcultures. Anthropologists have traditionally accepted the participant-observer role as an integral part of field research. This role has assumed many different characteristics, ranging from the anthropologist who tries to become a member of the group being studied, to the researcher who maintains an office to which informants come to be formally interviewed. Participating in an illegal urban subculture as a medium for research presents particular problems. The researcher who "participates" must do so in a transient manner that is always secondary to the role and image as a researcher. Maintaining the role as participant will render one's actions suspicious unless one is honest with the group about one's goals. If the role as researcher is revealed, one will be frequently checked and tested by the informant group, and maintaining the balance as an objective observer becomes difficult, as does remaining within the law.

In the subculture of prostitutes, one is not a participant short of becoming a prostitute. The traditional participant-observation is simply not possible in this environment: the researcher is in the way when the girls are conducting business, the nature of the business is intimate and observation demands participation, and the environment presents legal and safety problems.

In this situation, the participant-observer is likely to become a victim of the "conning" that is the art of the prostitute and code of the pimp, and the attitude toward such a participant, even if his/her interests are known, will be negative and possibly hostile. If the researcher decides "in the interests of science" to take on the role of prostitute, she takes the risk of value imposition based on her own rejection of the mores and the laws of the larger culture to which she subscribes.

For the researcher faced with these problems, the only reasonable solution to the restrictions inherent in full or peripheral participation is to use the opportunities provided by the jail, and the prostitute's social network and family ties. Accordingly, in the first context, the researcher can spend time in the jail both as a researcher and as a prisoner; in the second, the researcher can go to after-hours clubs and private parties with those in the "fast life"; third, the researcher can attend private parties attended by women in the "fast life"; and fourth, the researcher can meet relatives and close friends of the informants. Means

such as these are more realistic for the urban policy researcher than the textbook prescriptions of social research.

SUMMARY

Ethnographic research is clearly an invaluable information resource for agencies developing public policy recommendations. We have reached an impasse in our approach to social problems that requires an alternative perspective. Prostitution is a classic example of such an impasse. It is essential to ask the "actors" to reveal how they see their play instead of the audience or institutional managers. The ethnographer must, in turn, fully recognize his responsibility as a scientist in the public arena and the importance of accurate, objective data. The input of such ethnographic information into policy decisionmaking processes will produce more realistic solutions to public social problems.

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Workshop Summaries

Ethnography in Urban, Rural, and Suburban Settings

Recorders: **Jerry Mandel, M. Christine Kenty, and Terry Mason**

Despite the expectation that each workshop would develop distinctly different perspectives on the uses of ethnography for policymaking and planning in urban, suburban, and rural settings, a number of common themes emerged independently in all three groups. First, participants recognized that the present sources of data for developing sound and effective State plans are inadequate. Although the DAWN, CODAP, and national and local surveys are useful for addressing politically mandated questions, the reliance on incidence/prevalence and treatment data produces neither a description of the diverse drug-using populations nor an analysis of the drug-using patterns. Instead, it creates what one participant in the symposium called an "illusion of understanding," because these sources of data are unrepresentative and reflect primarily those people who use treatment facilities. Second, Single State Agency (SSA) representatives recognized a need for either changing or broadening their information source; they view ethnography as a promising alternative or supplement. In anticipating the role ethnography might play in drug planning and policymaking, the following uses emerged:

- As an early warning system that could alert State agencies to the emergence of new drug patterns;
- As a basis for providing another perspective on youthful drug user behavior;
- As a strategy for developing accurate information on patterns of community drug use;
- As a method of studying specific target or underserved groups, such as racial and ethnic minorities;

- As a basis for developing more effective prevention and education strategies; and

- As a preliminary means of ferreting out street patterns and practices that may require quantitative research.

Third, participants recognized that problems would arise in developing new relationships between ethnographers and SSAs. The following were identified as potential areas of difficulty:

- Ethnographers believed that they might be "used" by State agencies rather than having a genuine influence on policy development and State plans;

- State administrators believed that the advocacy component that is part of the research role of some ethnographers would be politically embarrassing;

- All participants recognized that presently there is a severe lack of minority-group ethnographers; and

- Participants realized that an infusion of new ethnographic techniques and resources would be met with resistance when they encroach on the established powers.

None of these problems were viewed as serious deterrents to the development of what participants in the 2-day symposium believed was the beginning of a new and exciting experiment. In keeping with this optimistic outlook, the following recommendations and plans were suggested:

- To communicate to ADAMHA officials the need to expand ethnographic research in the fields of alcoholism, drug abuse, and mental health;

- To develop requests for proposals on selected topics in order to further the utilization of ethnography in State planning;

- To develop a demonstration project that will show the utility of a national network of ethnographic field stations; and

- To develop a training program for ethnographers, particularly minorities, who will specialize in policy-related research.

THE URBAN WORKSHOP

OVERVIEW

The initial focus of this workshop was on the views and concerns of SSAs; the focus then shifted to the mutual interest of both groups. The basic questions were:

- What kinds of information do SSAs need in urban areas? What are the current sources, and what problems do SSAs encounter with traditional data sources?
- What additional information do SSAs want that could be obtained through ethnographic research?
- What difficulties would SSAs and ethnographers experience in working together? How could these be resolved?
- What are the types of alternative arrangements that could be developed by SSAs and ethnographers?
- What must be done to implement these arrangements?

PERSPECTIVES

There was general agreement that the basic sources of information used in State planning documents were inadequate to make a sensitive, quality assessment of drug abuse problems and responses. Furthermore, ethnography was viewed as having a major contribution to offer planners. There were differences, however, in emphasis and orientation between planners and ethnographers on how ethnography could best be utilized in urban areas.

Planners believed that it was important for them to rely on statistics and data on treatment populations. The ethnographers were more critical of these sources of information, and tended to see ethnography's value not only as a supplement to already existing data, but also as a primary research tool. The policymakers and planners recommended that ethnography be used specifically: (1) to furnish information for a first step in developing other research instruments; (2) as an early warning system for new drug problems; and (3) to deal with specific issues in specific locales. Ethnographers, however, viewed their value in more general terms, and envisioned themselves having continuous input and impact upon the system. The policymakers and planners were

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Participants: Urban Workshop. Joseph J. Bevilacqua, Elliot Bovelle, Adger Butler, Kenneth Budman, Gail Crawford, Christopher D'Amanda, Harvey W. Feldman, Karen Gunn, Jennifer James, Yosh Kawano, Mary Kloris, Jerry Mandel, Alberto G. Mata, Edward Preble, Irving K. Zola.

concerned about sampling misrepresentation because ethnographers study only small segments of the general population. Ethnographers, on the other hand, questioned the scientific validity of the more traditional research instruments and their capability of providing the more basic information needed for planning.

MAJOR THEMES

Traditional Data Sources

Despite wide variation in SSA staff size, functions, and the process used to develop annual plans, there was a consensus on why the plan is developed, what the key audience requires, the basic data used, and the numerous problems associated with the traditional data. The State plans are formulated in response to the requirements of legislators and funding agencies, whose primary requests are for measures of the extent of the drug problem (incidence/prevalence measures) and the extent of the response (the numbers of people in treatment and the utilization rates of treatment slots). SSAs rely on standard indicators to meet these demands. To measure the "problem," they use emergency room and medical examiner data, police records, heroin purity measures, and some macrosurveys; they use NIDA's Client Oriented Data Acquisition Process (CODAP) data to measure the utilization of treatment slots.

SSA delegates emphasized that this data is limited and fails to provide the kind of information needed to understand drug use problems at the local level. Policymakers believed that they have a political mandate to employ this data in producing incidence/prevalence estimates and must rely on the accuracy of reports. They concluded that the traditional data sources were necessary.

Possible Uses of Ethnography

There was general agreement that SSAs need insight into the drug use problems experienced in minority communities. Ethnography could retrieve data that now "falls between the cracks"---data on how users view treatment, their reasons for avoiding treatment, and treatment needs that are not addressed by traditional drug treatment agencies. Ethnography could provide an understanding of the cultural context in which drug use, criminal activity, and treatment take place.

Some workshop participants recommended specific study areas that ethnography could address:

- Test null hypotheses (in the market research mode): Does heroin use lead to crime? Are people avoiding treatment for specific reasons?

- Treatment avoidance: Why do those "on the street" underutilize treatment? Why is treatment generally rejected? Why does only a specific ethnic group reject treatment?

- Use of new drugs: Ethnography could provide a quicker and better barometer of new forms of drug abuse and suggest a range of reasonable responses. For example, DAWN and CODAP are slow to identify new drugs of abuse, and are often inaccurate; even when they identify new forms of drug abuse, the data offer no clue as to a proper response.

Some SSA staff envisioned a limited use of ethnography. For example, it was suggested that ethnography might be the first step in a two-stage data gathering process. Ethnographers would determine the proper questions to ask and the right people to query, and then a standard empirical instrument would be used. Similarly, other SSA personnel viewed ethnography as a tool to inform them of "what's really happening" so they could use traditional data sources more wisely.

Current SSA Use of Ethnography

Given their perceptions of the need to go beyond the traditional data, several SSAs have used ethnography to some extent, and others were seeking such information to help formulate their State plans. Illinois has employed a part-time ethnographer to bring planning staff into contact with the "real world" and has recently hired someone to study drug use in Latino communities. Philadelphia has hired a field researcher to compare ethnographic data with "archival data."

Possible Problems

Specific problems were raised. These included:

- How would ethnographic data be used by SSAs? Ethnographers were concerned that they would have little control over how the information will be used.

- Would the ethnographer run into difficulty when "street" information ran counter to the interests of policymakers?

- Could ethnographers, dealing intimately with small numbers and groups of people, provide representative and generalizable information?

- How can minority communities be studied when there is a lack of minority ethnographers?

Possible Applications of Ethnography

Several participants noted great variations between urban, suburban, and rural areas in drug use styles, and user populations, they also noted that there were significant variations within a single city. The participants concluded that wherever ethnography is applied, it would have to be adapted to the local situation. This does not seem to be a problem for trained ethnographers who are generally adept at moving into new areas to study different cultures.

There was a consensus that there were many styles of ethnography, with room for flexibility in the types of relationships between planners and ethnographers. The suggested styles are:

- Mobile field teams made up of "street people," sociologists, pharmacologists, etc., to "test" traditional data and to examine problems, such as: What does emergency-room data really tell us? What does a rush to use treatment slots in a particular community really mean?
- Stationary field teams to develop rapport and understanding in local situations over a period of time.
- Ethnographic outposts in various cities to determine trends or changes in patterns.
- Offer individual ethnographers a free hand in the community. This provides an opportunity to understand the cultural context of drug problems.
- Train "indigenous people" to produce ethnographic data--for example, a cadre of black and Spanish ethnographers to study drug use patterns in minority communities.
- Use ethnographers to link SSA and other top-level staffs to data sources that are overlooked, as the Illinois SSA is currently doing.
- Ask ethnographers to compare their data with other traditional data, as is being done in Philadelphia.

CONCLUSIONS AND RECOMMENDATIONS

Participants agreed that ethnographic research is a useful tool for planners. There was some discussion on the formal steps required to broaden the use of ethnography. Several SSA delegates believed it necessary to expand and formalize the network that was apparent at this conference as a means of guaranteeing that top-level policymakers recognized the value of this style of research.

THE SUBURBAN WORKSHOP

OVERVIEW

Of the 16 participants in this workshop, nearly half were researchers and half SSA representatives; some filled both roles. As part of the process of exploring ways to work together, both the SSA delegates and the ethnographers questioned each other about their respective work, what they need and want to do their jobs well, and what a joint enterprise might involve.

The SSA representatives expressed frustration with their present methods of gathering information. They said that they had become increasingly aware of the need for drug policy to reflect the reality of drug use on the street. They are interested in learning more about the patterns and dynamics of drug use, how drug use fits into other aspects of life, when and what intervention is called for, and what new drugs are coming into use. They also need to be informed of new trends so as not to be caught off guard by publicity about new drugs.

For their part, ethnographers want to develop systematic ways to share their information with government authorities. During the past decade, their research had revealed very different pictures of drug use than those promulgated by legislators, drug authorities, and other researchers. Ethnographers hope that better information, gathered through close, long-term relationships with drug users both in and out of treatment and without enforcement overtones, might lead to more realistic and effective policies.

Ethnographers want to develop a sharing network of researchers seeking similar information at many sites. Ethnography can be a lonely, solitary task. Researchers fear that they sometimes lose their objectivity because they have no one to talk to about their work. They need support, stimulation, and criticism from fellow workers. In addition, it is most important to cross-validate findings from one site to the next, and to extend the limits of research beyond the capacity of one individual.

MAJOR ISSUES

SSA Concerns

Even in their general mood of receptiveness, SSA representatives were careful to emphasize certain limits on their use of ethno-

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Participants: Suburban Workshop. Katherine Carlson, Patricia Cleckner, Eugenia Flaherty, Richard Freeman, John Gustafson, Judy Holland, James Inciardi, Bruce Johnson, M. Christine Kenty, Eugene Rinaldi, Richard Russo, Alden Small, Laurie Townsend, Dan Waldorf, James Walters, Wayne Weibel, Donald Zappone.

graphic data. Any new research strategies must fit into the complex structures that already exist and will most certainly meet with resistance when they encroach on established power.

Participants pointed out that the network of drug treatment agencies began long before SSAs were organized and, in some sense, the tail wags the dog. Treatment programs are powerful and often inflexible; both they and the communities in which they are located resist changes that might threaten their existence. Reallocating some treatment slots is relatively simple, but large-scale policy changes produce political furor.

State legislatures also have firm ideas about drug abuse and the proper method of handling drug users, often based on their constituents' demands. In many States, legislators and officials will simply ignore, distort, or simplify beyond recognition data that is contrary to their beliefs. They also tend to allocate funds on the basis of who "yells the loudest" or exerts the most pressure. And in large part, drug treatment and law enforcement are based on sumptuary laws, and support a moral order and a set of assumptions that are loathesome to many academicians. SSA staff may be willing to hear heretical concepts about how functional drug abusers are, for instance, but legislatures and treatment specialists may not want to hear them.

The SSA delegates emphasized their need to maintain control of the information ethnographers deliver. They want to take the data, mingle it with other sources, then develop their own policies and work with political forces around them; the ethnographer would serve only as an advisor in the process.

SSA participants in the group sought assurances that advocacy is not a necessary and omnipresent aspect of ethnography. The ethnographers assured them that advocacy is actually rare and is entirely a function of the individual researcher, although ethnographers often become close to the groups they study and develop an understanding of the appropriateness or inappropriateness of intervention.

Ethnographer Concerns

Although ethnographers are eager to contribute to policy, they are still protective about the use of the information they generate and the conditions under which they undertake their studies. Although an ethnographer could answer a straightforward question like, "Is PCP use increasing?," a participant stated emphatically that no ethnographer would stop there. Ethnographers do not want only to deliver information; they do not want the context and depth of their data ignored or discarded in the translation to public policy. Ethnography is concerned not only with counting the numbers of drug users, but also with drawing a portrait of what drug users do, how they do it, and how they perceive it. Ethnographers will go on to supply all this information.

Developing relationships with respondents and learning enough about a group to begin understanding their social structure and symbolic behavior requires a great deal of time. Although not all ethnographic research needs to be full-scale ethnography, ethnographers feel a need to guard against projects that will generate conclusions without adequate time and support for fieldwork and thoughtwork, or without adequately educated and experienced ethnographers.

The group carried on a spirited discussion about the use of ethnographic information available from sources, other than ethnographers, who are close to the street, including police and parole officers, treatment program staff and clients, students, and teachers. They also discussed the use of graduate students for short-term studies. There was general agreement that much valuable information is being obtained from these sources. Nonetheless, some ethnographers believe that these existing sources can be used effectively only under the direction of a skilled ethnographer who will fashion the network, verify information across multiple sources, and organize and categorize data.

Ethnographers are trained to approach groups without prejudices and preconceptions (to the extent this is possible) and to allow respondents to express their own view of their lives. This perspective is distinctively different from other information sources, and yields unique data. SSAs and ethnographers will want to discuss this issue further as they plan joint ventures.

In addition, many ethnographers are loath to deliver information about drug use to law enforcement agencies, who might want to understand drug users only to punish them effectively. Ethnographers want their knowledge to be put to humane uses.

Possible Applications of Ethnography

The group suggested many ways that ethnographic research could be useful to SSAs as well as to national and local authorities. Traditionally, ethnographers have concentrated on a single culture or subculture, and have sought to understand and explain all facets of group life. Such studies have been done on various drug-using groups, and could certainly be done on many more. Researchers could develop portraits of street life in any locale, focusing either on a particular drug or on the entire range of substance use in its social context. Similarly, the use of one substance could be compared in different locales, as was done in the PCP study discussed earlier. Ethnographers could answer particular questions in the course of their work--for example, seeking to understand why some users avoid treatment.

Such information about drug use and drug users could be applied in planning allocations, as well as in developing new treatment methods. One SSA representative suggested that new informaton could most easily be absorbed into prevention strategies, thus avoiding the inflexibility of treatment funding and philosophies.

The group agreed on the potential benefit of an ethnographic "early warning system" to spot new drugs as they emerge and to provide information on their use and effects before they cause medical crises or citizen panics. Ethnographers working on the streets were well aware of the widespread use of both quaaludes and PCP long before this information hit the press and cries went up for governmental intervention. If researchers were conducting ongoing studies in carefully selected areas, they could keep authorities apprised of experimental substances being added to the street pharmacopeia. Drug agencies might then be better able to plan intervention if it were necessary or to put horror stories in perspective if they appeared. Similarly, agencies could respond quickly to requests for information from legislators or officials on the use of a new drug. An additional benefit from a network of ongoing research sites might be an increased understanding of the diffusion patterns of drugs--where new drugs surface and along what paths they travel from group to group and city to city.

The ethnographers in this workshop had used their skills to evaluate drug treatment programs and are convinced of the utility of the method. They stated that they can study the interactions between staff and clients, can tease out value differences that interfere with or constitute treatment, and can understand the hidden rules that organize an agency and its treatment regimen. Experienced evaluators have learned that by sharing their insights during the course of the evaluation, thereby helping staff and clients clarify their roles, rather than giving a summative evaluation. Thus, they can enable programs to improve their operations as a part of the evaluation.

The group also explored how ethnography might be used in conducting long-term followup studies of clients leaving treatment.

Finally, it was pointed out that ethnographers can investigate the community processes that keep drug programs in place, the political processes involving drug issues, and the interactions among institutions that make drug use their concern. Ethnographers study the drug use of an individual as one part of their experience. They also can cast the drug treatment and law enforcement system in a wider context.

Proposed Models for SSA Ethnographic Research

Before concluding the workshop, ethnographers and SSA representatives tried to envision various working relationships between government agencies at all levels and various sources of ethnographic information. These suggested models are only skeletal; not only do many unsolved theoretical problems remain, but models and methods must be adapted to meet the needs and circumstances of each State.

One of the many uncertainties is the question of research sites. DAWN data, according to one ethnographer, suggests that drugs

emerge in cities and radiate through the suburbs into rural areas. This would suggest that placing ethnographers solely in major metropolitan areas could provide an adequate "early warning system." However, the group generally agreed it would be premature at this point to exclude the latter two sites from ethnographic investigation. The perennial question of how to use limited resources remained unsolved.

The group considered what an ethnographic field station would look like in a suburban area. In dense urban areas, a storefront can provide a permanent base for an ethnographer studying the inhabitants of an area of a few blocks. In contrast, a suburban researcher, while still needing a telephone contact point for respondents, will most likely work from a car that carries him or her to many separate and distant hangouts.

SSAs working in the suburbs might choose to use their resources to support full-time, independent ethnographers immersed in carefully selected subcultures for extended periods. These ethnographers could generate data on emerging drugs, as well as detail street life, drug users, and their drugs of choice. These workers also could test various hypotheses about drug use. Ethnographers can make initial contacts in treatment programs, parlay those contacts into a street network, or gain entree in any number of ways in which they are skilled.

SSAs also might choose to hire an ethnographer as part of their research staff. This employee could conduct both fieldwork and other research tasks.

A more flexible and broader concept involves an SSA supporting one full-time or part-time ethnographer expressly to cultivate a network of secondary ethnographic sources. Over time, the ethnographer could locate and develop close contacts with any number of people throughout the State who have access to street drug users. These secondary sources might include police, parole officers, special narcotics officers, treatment staff, ethnographers working in the field, academic drug specialists privy to campus drug activity, and even anthropology or sociology graduate students engaged in their own small-scale research. By comparing information and cross-validating data among a full range of such contacts, a skilled ethnographer should be able to piece together an accurate picture of drug use in the State, perhaps one that stretches beyond the margins of one individual working in a limited area. Some of the workshop participants believed that such a network might be assembled even without an employed ethnographer to supervise it, especially if the area contained ethnographers already familiar with many groups and willing to supervise graduate students.

Finally, an SSA might organize other agencies to join together in the support of one ethnographer in a given locale. An ethnographer studying an area could generate social information important to criminal justice, health, and drug/alcohol agencies, for instance. This kind of full-scale, multi-issue investigation, carried on over

a period of time and perhaps in many sites throughout the country, could be an exciting stepping stone to an ethnographic network that is not limited to drug studies alone.

THE RURAL WORKSHOP

PROBLEMS

This workshop began with a discussion of the potential difficulties that ethnographers and drug policymakers must face and resolve to work together effectively. Most of these issues concerned intellectual, ethical, and stylistic conflicts that could emerge between the two groups; most were introduced by ethnographers.

Some of the ethnographers were concerned about the possible undesirable effects that their data and research efforts might have on the populations studied. Because drug use is often an illegal activity, it would be important for the ethnographers to know who would have access to their data, especially if law enforcement agencies would be privy to this data. It was suggested that this might pose a particular problem in rural areas where drug users could be more easily identified than in urban areas.

Workshop participants agreed that ethnographers would need to make sure that drug abuse professionals understood the need to protect the confidentiality of their research subjects and to determine, through contractual agreement, conditions on how the data will be used, terms of access to it, etc.

This issue was part of a larger concern regarding the degree of autonomy that ethnographic researchers would be allowed by policymakers, given the relatively unstructured and independent nature of ethnographic research. There were also questions about the working style of ethnographers and the degree to which they would be in conflict with such norms of an administrative office such as hours and dress styles. A question also was raised about the need for ethnographers to specify the specific skills and contributions they could bring to agencies and programs.

The question of what role an ethnographer can be expected to play in determining what constitutes a drug problem also was discussed. This poses a problem because ethnographers may provide data and perspectives that challenge policymakers' accepted definitions of what constitutes a drug problem.

Participants: Rural Workshop. Michael Agar, Carl Akins, Patricia Casey, John French, Robert Garcia, Richard Gonzalez, Dean Gerstein, Phillip Hall, Carl Klockers, William Kornblum, Elliot Liebow, Terry Mason, Joan W. Moore, Alfred Roberson, Arthur Rubel, Mary Tijerina, Andrew Walker.

A number of ethnographers and one representative of an SSA stressed the importance of funding ethnographic studies of all levels of policymaking in order to provide an understanding of the complete drug use/drug treatment system. This is part of the gestalt approach to understanding problems that characterize ethnographic research and was suggested as an important component of the contributions of ethnography to the policy process. However, some SSA representatives expressed the view that this would be politically threatening to agencies and would be given a low priority as a research area. Federal agencies have done these types of studies, however.

POSSIBLE APPLICATIONS OF ETHNOGRAPHY

There was a general consensus among the SSA participants about the limited utility of the data they currently use to develop their State plans. They agreed that data was particularly lacking about rural areas, where even traditional sources are less reliable or nonexistent. The SSAs need a better understanding of the communities served both by them and related drug programs, particularly in rural areas. This is not limited to types of drugs and drug-use patterns, but includes the need for understanding the culture and its values.

State planners believe the planning process tends to be ad hoc, based on limited and static data on drug use and often responding to crises defined by the media and by political pressure. A more continuous and complete source of information on the nature and extent of drug use in different communities would provide a basis for more rational plans, and would aid planners in responding to crises. Ethnographers could provide a broad-based system analysis of communities, with continuous information on changes in drug-use patterns to help planners anticipate problems.

There are also types of drug-use problems that planners learn about informally, such as polydrug use by middle-class housewives, that they cannot include in their State plans because no systematic data is available. Ethnographers could provide research on these problems and define similar new target groups that have not been previously considered by policy planners, although it should be stressed that there are questions about the ability of ethnographers to determine what is a drug problem.

Participants agreed that treatment and prevention models are most often designed for urban areas and are inappropriate for rural populations. They believed that ethnographers might be particularly useful in formulating models more appropriate to rural communities.

There is little information available on drug abusers who do not get into treatment. This is another area in which ethnographers may be useful because they have the capability of determining the problems and needs of drug users who are not in treatment. Ethnographers are also in a position to determine whether the

treatment resources are being used by the groups who need them most. There is also a need for followup information on clients who leave programs to return to their communities. Ethnography is particularly suited for assessing the status and well-being of clients after they leave treatment.

Federal guidelines, rules, and allocations of funds have a strong influence on the planning process. Participants in the rural workshop believed that there must be more emphasis on the uniqueness of rural communities and the kinds of problems and needs presented in these areas. In the past, various rural areas have been under pressure to demonstrate the same kinds of drug problems found in urban communities. Ethnographic research should be encouraged in the planning process because it can help to assess these unique patterns of drug use in rural areas. For example, marijuana use may not be considered a serious drug problem in the community of Harlem in New York City in light of the heroin abuse prevalent there. In rural communities, however, marijuana may be the only drug available and may be perceived by some community members as a serious problem, particularly for the very young user.

POSSIBLE RESEARCH METHODOLOGIES

Although specific research methods were discussed only briefly and in general terms, the ethnographers offered several methodological suggestions. Research approaches and methodologies used by ethnographers range from relatively unstructured holistic approaches to hypothesis testing and use of quantitative techniques.

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